Income volatility, taxation and the functioning of the U.S. labor market*

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Abstract

The goal of this report is to characterize income volatility in the U.S. labor market and examine its causes and consequences. To achieve this goal, we analyze U.S. business and household tax records. These administrative data sets allow us to match employees and employers and to construct panel data on the outcomes and characteristics of U.S. firms, individuals and households. The main insights from the empirical analysis may be summarized in four broad conclusions. First, income volatility rose steadily during 2001-2009, peaked during the Great Recession, then dropped during 2010-2015. However, these national averages miss a lot. Income volatility is relatively high on the coasts and in the Western part of the country, and lower socioeconomic areas tend to have higher income volatility. Second, income volatility is lower if one considers income net of taxes and transfers. In particular, the Federal tax-transfer system attenuates both permanent shocks at the worker level and the pass-through of firm shocks to workers' earnings. Third, worker mobility across firms generates relatively small changes in income. By contrast, sorting of better workers to better firms and the exit and entry of firms in local markets are empirically important determinants of workers' income. Fourth, income volatility, at the individual and market level, may generate substantial changes in tax payments and the receipt of tax credits. This indicates that income volatility in the U.S. labor market could make it difficult to obtain accurate predictions of tax revenues.

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1 Introduction

The aim of this report is to characterize income volatility in the U.S. labor market and examine its causes and consequences. There are a number of key questions addressed. What is the distribution of income volatility in the U.S. labor market? How large and persistent are the year-by-year changes in the incomes of American workers? How much does the sorting of workers to firms, regions, and industries matter for income volatility? How do the estimates of income volatility change when we account for other sources of income such as spousal earnings? To what extent does the Federal tax-and-tranfer system affect measures of income volatility? Does income volatility make it difficult to predict tax revenues or receipt of tax credits?

Data challenges have made it difficult in the past to answer these questions. The ideal data covers a large number of individuals, includes a sufficient number of years on each individual, links each individual to her employer, and links individuals to households. While such data has not previously been available for the U.S., administrative data has provided such information for existing studies on some other countries. The advantages of these administrative data sets are the accuracy of the income information provided, the large sample size, and the lack of attrition, other than what is due to migration and death, as well as the possibility to link to employers and households.

To investigate the above questions, we analyze U.S. business and household tax records. These administrative data sets allow us to match employees and employers and to construct panel data on the outcomes and characteristics of U.S. firms, individuals and households. The main insights from the empirical analysis may be summarized in four broad conclusions. First, income volatility rose steadily during 2001-2009, peaked during the Great Recession, then dropped during 2010-2015. However, these national averages miss a lot. Income volatility is relatively high on the coasts and in the Western part of the country, and lower socioeconomic areas tend to have higher income volatility. Second, income volatility is lower if one considers income net of taxes and transfers. In particular, the Federal tax-transfer system attenuates both permanent shocks at the worker level and the pass-through of firm shocks to workers' earnings. Third, worker mobility across firms generate relatively small changes in income. By contrast, sorting of better workers to better firms and the exit and entry of firms in local markets are empirically important determinants of workers' income. Fourth, income volatility, at the individual and market level, may generate substantial changes in tax payments and the receipt of tax credits. This indicates that income volatility in the U.S. labor market could make it difficult to obtain accurate predictions of tax revenues.

Our work relates to a considerable literature on income volatility, risk, and inequality.[1] DeBacker et al. (2013) use a panel of tax returns to study the persistent-versus-transitory nature of rising inequality in individual male labor earnings and in total household income, both before and after taxes, in the U.S. Their paper is the first to estimate error components models of income dynamics using U.S. administrative data.[2] Building on this work, we characterize the

[1]See, for example, the recent review by Meghir and Pistaferri (2011), and the extensive list of studies referenced therein.

[2]See also Blundell et al. (2015) who perform a similar analysis for Norway.

variation over time and across areas in income volatility in the U.S. and explore the factors correlated with high income volatility. Moreover, we separate between income volatility caused by idiosyncratic shocks to individual workers and the volatility reflecting firm shocks common to workers in that firm. We also explore how the sorting of workers to firms, regions, and industries matters for the volatility and inequality in income. Our report also adds to existing work in that we compare volatility in earnings, household gross income and household net income. This allows us to draw inference about how the family and the tax-transfer system attenuate income volatility.

Our analysis also contributes to a large and growing literature on firms, income volatility and labor market inequality, reviewed in Card et al. (2018). A number of studies show that trends in wage dispersion closely track trends in productivity dispersion across industries and workplaces (Faggio et al., 2010; Dunne et al., 2004; Barth et al., 2016). While this correlation might reflect that some of the productivity differences across firms spill over to wages, it could also be driven by changes in the degree to which workers of different quality sort into different firms (see e.g. Murphy and Topel, 1990; Gibbons and Katz, 1992; Gibbons et al., 2005). To address the sorting issue, a growing body of work has taken advantage of matched employer-employee data. Some studies use this data to estimate the pass-through of changes in the value added of a firm to the wages of its workers, while controlling for time-invariant firm and worker heterogeneity (see e.g. Guiso et al., 2005; Card et al., 2013a; Card et al., 2018; Carlsson et al., 2016; Lamadon, 2016; Friedrich et al., 2019). These studies typically report estimates of pass-through in the range of 0.05-0.20. We complement this work by providing evidence of pass-through for a broad set of firms in the U.S. and by showing how the estimated pass-through of firm shocks is confounded by market shocks and attenuated by the tax-transfer system.

Another set of studies use the matched employer-employee data to estimate the changes in earnings caused by workers moving across firms. Following Abowd et al. (1999), these studies typically use an additive worker and firm effects model. They tend to conclude that firms play an important role in the determination of earnings, with a typical finding that about 15-20 percent of the variance of log earnings is attributable to the choice of firm (Card et al., 2018). We show, however, that firm effects are small in the U.S. labor market, explaining only a few percent of the variation in earnings. This finding contrasts with recent work from the U.S. (Sorkin, 2018; Song et al., 2018) as well as many studies from other developed countries (Card et al., 2018). The reason is that these studies do not address the concern that estimates of firm effects will be biased upward and estimates of worker sorting will be biased downward in finite samples, with the size of the bias depending inversely on the degree of worker mobility among firms (Andrews et al., 2008). Following recent work by Bonhomme et al. (2019) and Kline et al. (2018b), we apply two alternative approaches to correct for the bias of the estimator of Abowd et al. (1999). Both approaches show that firm effects explain very little of the variation in earnings in the U.S. economy, once one corrects for bias due to limited mobility. Instead, a substantial part of the variation in earnings is due to positive sorting of high wage workers to high paying firms. Our report also differs in that we estimate the additive worker and firm effects model both for earnings, household gross income and household net income. This allows us to draw inference about how the progressive nature of the tax-transfer system attenuates the income changes associated with moving across firms and reduces the incentives of better workers to sort into better firms.

The remainder of the report is organized as follows. Section 2 describes the data and the sample selection. Section 3 characterizes income mobility in the U.S. labor market and describe how it varies over time and across areas. In Section 4, we use several complementary approaches to examine causes and consequences of income volatility. Section 5 offers some concluding remarks.

2 Data sources and sample selection

2.1 Data sources

Our empirical analyses are based on a matched employer-employee panel data set with information on the characteristics and outcomes of U.S. workers and firms. This data is constructed by linking U.S. Treasury business tax filings with worker-level filings for the years 2001-2015. Below, we briefly describe data sources, sample selection, and key variables, while details about the data construction and the definition of each of the variables are given in Appendix A.

Business tax returns include balance sheet and other information from Forms 1120 (C-corporations), 1120S (S-corporations), and 1065 (partnerships). The key variables that we draw on from the business tax filings are the firm's value added, commuting zone, and industry code. Value added is the difference between receipts and the cost of goods sold. Commuting zone is constructed using the ZIP code of the firm's business filing address. Industry is defined as the first two digits of the firm's NAICS code. We define a market as the combination of an industry and a commuting zone. At times we will aggregate these markets according to the combination of Census regions (Midwest, Northeast, South, West) and broad sectors (Goods and Services). We will refer to this classification as "broad markets".

Earnings data are based on taxable remuneration for labor services for direct employees and independent contractors. Earnings include wages and salaries, bonuses, tips, exercised stock options, and other sources of income deemed taxable. These forms are filed by the firm on behalf of the worker and provide the firm-worker link. Gross household income is constructed using a definition similar to that of Piketty and Saez (2003). Net household income is given by gross household income minus Federal taxes plus Federal benefits from Social Security and unemployment. See Appendix A for further details.

We express all monetary variables in 2015 dollars, adjusting for inflation using the Consumer Price Index.

2.2 Sample Selection

In each year, we start with all individuals aged 25-60 who are linked to at least one employer. Next, we define the worker's firm as the EIN that pays her the greatest direct (W-2) earnings

	Workers		Firms	
Panel A.	**Baseline Sample**			
	Unique	**Observation-Years**	**Unique**	**Observation-Years**
Full Sample:	89,570,480	447,519,609	6,478,231	39,163,975
Panel B.	**Movers Sample**			
	Unique	**Observation-Years**	**Unique**	**Observation-Years**
Movers Only:	32,070,390	207,990,422	3,559,678	23,321,807
Panel C.	**Stayers Sample**			
	Unique	**6 Year Spells**	**Unique**	**6 Year Spells**
Complete Stayer Spells:	10,311,339	35,123,330	1,549,190	6,533,912
10 Stayers per Firm:	6,297,042	20,354,024	144,412	597,912
10 Firms per Market:	5,217,960	16,506,865	117,698	476,878

Table 1: Overview of the Sample

Notes: This table provides an overview of the full sample, movers sample, and stayers sample, including the steps involved in defining the stayers sample.

in that year. This definition of a firm conforms to previous research using the U.S. business tax records (see, e.g., Song et al., 2018). The EIN defines a corporate unit for tax and accounting purposes. It is a more aggregated concept than an establishment, which is the level of analysis considered in recent research on U.S. Census data (see, e.g., Barth et al., 2016), but a less aggregated concept than a parent corporation. As a robustness check, we investigated the sensitivity of the estimated firm wage premiums to restricting the sample to EINs that appear to have a single primary establishment. These are EINs for which the majority of workers live in the same commuting zone. It is reassuring to find that the estimated firm wage premiums do not materially change when we use this restricted sample.[3]

Since we do not observe hours worked or a direct measure of full-time employment, we follow the literature by including only workers for whom annual earnings are above a minimum threshold (see, e.g., Song et al., 2018). In the baseline specification, this threshold is equal to \$15,000 per year (in 2015 dollars), which is approximately what people would earn if they work full-time at the federal minimum wage. As a robustness check, we investigate the sensitivity of our results to other choices of a minimum earnings threshold. We further restrict the sample to firms with non-missing value added, commuting zone, and industry. The full sample includes 447.5 (39.2) million annual observations on 89.6 (6.5) million unique workers (firms).

In parts of the analysis, we consider two distinct subsamples. The first subsample, which we refer to as the *stayers sample*, restricts the full sample to workers observed with the same employer for eight consecutive years. This restriction is needed to allow for a flexible specification of how the worker's earnings evolve over time. Specifically, we omit the first and last years of these spells (to avoid concerns over workers exiting and entering employment during the year, confounding the measure of annual earnings) and analyze the remaining six-year spells. Furthermore, the stayers sample is restricted to employers that do not change commuting zone

[3] In the baseline sample, the AKM (BLM) estimates of firm effects are around 10 (3) percent. By comparison, the restricted sample gives AKM (BLM) estimates of approximately 9 (3) percent.

or industry during those eight years. Lastly, we restrict the stayers sample to firms with at least 10 such stayers and markets with at least 10 such firms, which helps to ensure sufficient sample size to perform the analyses at both the firm and the market level. The stayers sample includes 35.1 (6.5) million spells on 10.3 (1.5) million unique workers (firms).

The second subsample, which we refer to as the *movers sample*, restricts the full sample to workers observed at multiple firms. That is, it is not the same EIN that pays the worker the greatest direct (W-2) earnings in all years. Following previous work, we also restrict the movers sample to firms with at least two movers. This restriction might help reduce the limited mobility bias. It also makes it easier to directly compare the AKM and BLM estimates of firm effects to those produced by the approach of Kline et al. (2018b) (which requires at least two movers per firm). The movers sample includes 32.1 (3.6) million unique workers (firms).

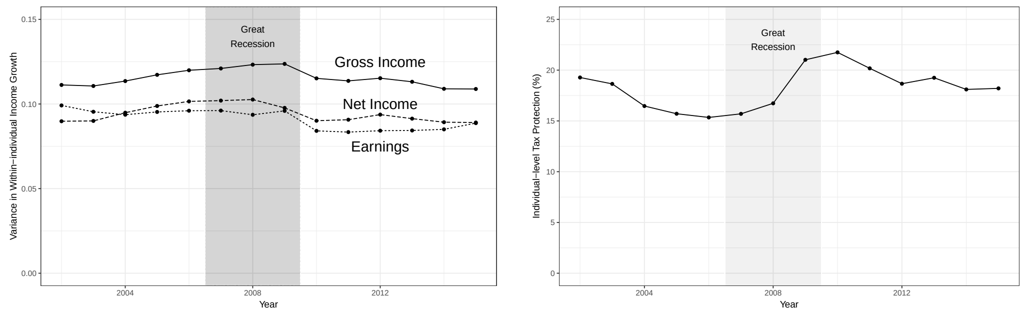
Table 1 compares the size of the baseline, the stayers, and the movers samples. Detailed summary statistics of these samples of linked firms and worker are given in Appendix Table A.1. The samples are broadly similar, both in the distribution of earnings but also in firm-level variables such as value added, wage bill, size, and the geographic distribution across regions and sectors. The most noticeable differences are that the stayers have, on average, somewhat higher earnings and tend to work in firms with higher value added.

3 Income volatility in the U.S.

In this section, we characterize income volatility in the U.S. labor market and describe how it has changed over time and across areas. Following the literature, volatility is defined as movements up or down in a household's income over time, as measured by the variance of the year-by-year changes in log household (annual) income. We construct this measure of volatility for three different measures of income: individual earnings, gross household income, and net household income. Following Blundell et al. (2015), we interpret the reduction in net household income volatility relative to gross household income volatility as a measure of the protection provided by the federal tax-transfer system. The transfers include Social Security benefits, unemployment benefits, and the Earned Income Tax Credit (EITC).

Time trends in income volatility

Figure 1 presents estimates of income volatility across years. In Figure 1(a), we see that gross income volatility rose steadily during 2001-2009, peaking during the Great Recession, then dropped during 2010-2015. Net household income and earnings volatility followed similar trends, but with smaller magnitudes. Figure 1(b) presents one minus the ratio of net household income to gross household income, multiplied by 100%. This is a measure of the reduction in income volatility, or protection, that is due to Federal taxes and transfers. The Federal tax-transfer system provides substantial protection against income volatility. Federal taxes and transfers reduce income volatility by about 21% on average, with a low of around 15% in 2001 and a peak of around 23% in 2009.



(a) Earnings, Gross and Net Household Income Volatility

(b) Percentage of Gross Income Volatility Attenuated by the Federal Tax-Transfer System

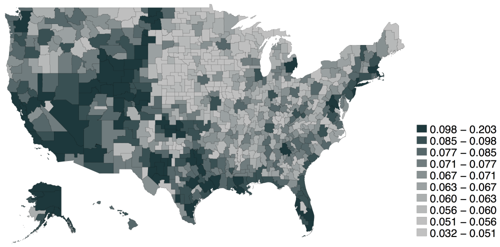
Figure 1: Income Volatility in the United States

Notes: This figure presents (a) the quantity of income volatility in the United States for gross household income, net household income, and earnings, and (b) the percentage reduction in volatility attributable to the federal tax-transfer system. Volatility is defined as movements up or down in a household's income over time, as measured by the variance of the year-by-year changes in log household (annual) income. The volatility reduction due to the federal tax-transfer system is defined as 100% multiplied by one minus the ratio of net household income volatility to gross household income volatility.

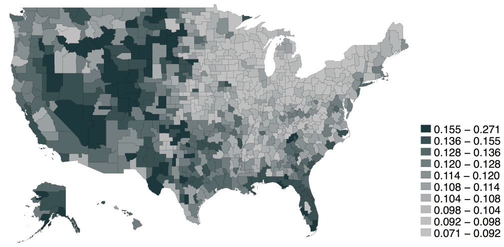
Geographical variation in income volatility

In Figure 2, we present the geographic distribution of the volatility measure when measured separately for each commuting zone in the United States. We see that income volatility tends to be higher on the coasts and in the Western part of the country, but lower in the Midwest and along the Great Lakes. The patterns are broadly similar across income definitions with a correlation of 0.78 between measures of local income volatility in earnings and gross household income and a correlation of 0.99 between between measures of local income volatility in gross and net household income.

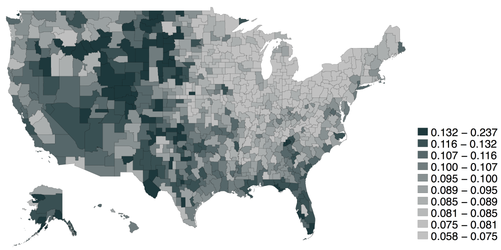
Figure 3(a) presents correlations between gross income volatility and other local socio-economic conditions within the commuting zone, where the measures of commuting zone conditions are from Chetty et al. (2015). Figure 3(b) presents these correlations for net income volatility. Correlations are presented in absolute value, with the sign of the correlation indicated with a symbol of (+) for positive or (-) for negative. Overall, income volatility tends to be larger in areas that are worse on other measures of economic and social conditions. Among economic conditions, income volatility is most positively correlated with local income inequality (as measured by the Gini coefficient) and the local poverty rate. It is negatively related to labor force participation and the fraction of the population with income between the 25th and 75th percentile (a proxy for the middle class). Among social conditions, income volatility is most negatively related to the social capital index (which measures social resource availability) as well as to our measures of short commute time to work, the marriage rate and the college graduation rate. Moreover, local income volatility is positively related to the measures of violent crime rate, segregation experienced by the impoverished, and the high school drop out rate.



(a) Earnings Volatility



(b) Gross Income Volatility



(c) Net Income Volatility

Figure 2: Geographic Distribution of Earnings and Income Volatility

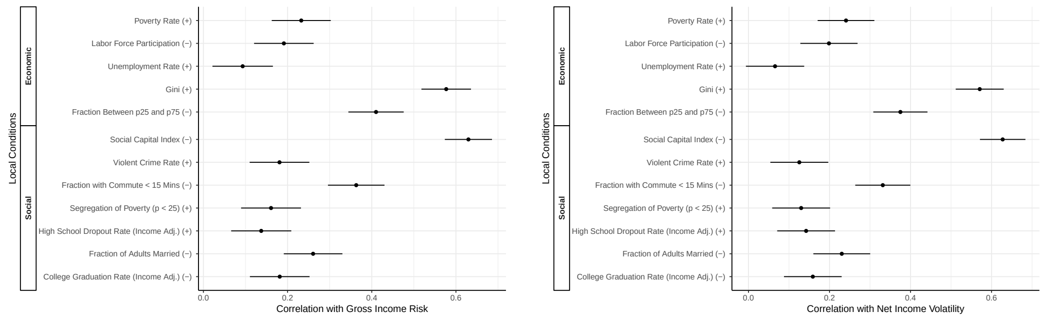
Notes: These figures present the geographic distribution of volatility in individual earnings, gross household income, and net household income. Volatility is defined as movements up or down in a household's income over time, as measured by the variance of the year-by-year changes in log household (annual) income.

4 Causes and consequences of income volatility

In this section, we use several complementary approaches to examine causes and consequences of income volatility.

4.1 Income processes

In this subsection we follow a literature which explores income volatility by estimating a statistical process of income (see e.g. Blundell et al., 2015, Meghir and Pistaferri, 2011). These analyses use the stayers sample. The estimated income process permits a decomposition of the measure of income volatility into various components that capture different sources of volatility. Appendix B.1 lays out and explains the income process and shows how it is identified and estimated. As explained in this appendix, one component of the income process reflects the trends in income over time and across ages that are common to households, whereas the remaining volatility in income can come from at least three sources: idiosyncratic permanent shocks to the worker's income; idiosyncratic transitory shocks to the worker's income; and changes in the firm's performance or productivity (as measured by the value added) that are passed on to workers within that firm.



(a) Gross Income

(b) Net Income

Figure 3: Geographic Correlates of Income Volatility

Notes: This figure presents the correlations between (a) local gross household income variability and other local conditions, and (b) local gross household income variability and local conditions. The local conditions are divided into economic and social categories. The data on commuting zone conditions is from Chetty et al. (2015). Commuting zones are weighted by the number of FTE worker observations with valid gross household income measures when estimating the correlations and their standard errors. Correlations are presented in absolute value, with the sign of the correlation indicated with a symbol of (+) for positive or (-) for negative.

Results from baseline model

Appendix B.2 presents the parameter estimates, and reports results from several specification checks. In Table 2 under the column labeled "Firm Only," we summarize the estimation results from the baseline model and use these estimated parameters to quantify the contribution to income volatility from the various sources. The full set of parameter estimates is available in Appendix Table A.2. The standard deviation in log earnings growth is 0.17. Decomposing the variance in log earnings growth, we find that almost 40 percent is due to permanent shocks at the worker level, 58 percent can be attributed to transitory shocks at the worker level, and 3 percent is due to the pass-through of permanent shocks to value added at the firm level .

The estimated pass-through rate $\hat{\gamma}$ is 0.14, suggesting that a 10 percent permanent increase in the value added of the firm leads to a 1.4 percent permanent increase in the earnings of incumbent workers. This pass-through rate is in the range of estimates reported by Card et al. (2018). While permanent shocks to value added are transmitted to workers' earnings, transitory firm shocks are not. This finding is consistent with previous work (see e.g. Guiso et al., 2005; Friedrich et al., 2019). A natural interpretation of this finding is that transitory changes in value added reflect measurement error that does not give rise to economic responses. In the remainder of the report, we will treat the transitory changes in value added as measurement error and focus on the pass-through of the permanent shocks.

We repeat the analysis for gross household income and net household income. Appendix Figure A.3 summarizes the estimation results and uses the estimated parameters to quantify their contributions to volatility in gross and net household income. The standard deviation in log gross income growth is 0.23, while it is 0.21 for log net income growth. We find that 33 percent of gross income volatility is due to permanent shocks at the worker level, 65 percent can be attributed to transitory shocks at the worker level, and 1 percent is due to the pass-through

	Parameters and Growth Decomposition			
	Firm Only		Accounting for Markets	
	Parameter	Var. (%)	Parameter	Var. (%)
Permanent Worker Shock (Std. Dev.)	0.10 (0.00)	39.5%	0.10 (0.00)	38.1%
Transitory Worker Shock (Std. Dev.)	0.13 (0.00)	57.6%	0.13 (0.00)	57.4%
Permanent Firm Shock Passed-through (Std. Dev.)	0.03 (0.00)	2.8%	0.02 (0.00)	1.8%
— Permanent Firm Shock Passthrough Coefficient	0.14 (0.01)		0.13 (0.01)	
Transitory Firm Shock Passed-through (Std. Dev.)	0.00 (0.00)	0.0%	0.00 (0.00)	0.0%
— Transitory Firm Shock Passthrough Coefficient	-0.01 (0.01)		0.00 (0.00)	
Market Shock Passed-through (Std. Dev.)			0.02 (0.00)	1.1%
— Market Shock Passthrough Coefficient			0.18 (0.02)	

Table 2: Variance Decomposition in Baseline Passthrough Specification

Notes: This table presents the variance decomposition of the joint process for growth in value added and earnings in the baseline specification. In the estimation sample, the standard deviation of log value added growth is 0.31, and the standard deviation of log earnings growth is 0.17. The estimated moving average coefficients are used to construct the variances of transitory shocks. The full set of parameter estimates is available in Panel A of Appendix Table A.2.

of permanent shocks to value added at the firm level; these percentages are very similar for log net household income. We also find pass-through rates of firm permanent shocks to gross and net household income of 0.136 and 0.127, respectively. This indicates that the progressive nature of the Federal tax-transfer attenuates more than 10 percent of the economic consequences of a firm shock.

Contribution of firms versus markets

We have thus far followed the existing literature in assuming that all income volatility is specific to the individual or the household, abstracting from aggregate regional or industry shocks. We now take two steps to distinguish between shocks that are specific to individual workers versus those that are common to workers in a market.

First, we estimate the earnings and value added processes conditional on a full set of year times market fixed effects. The results are presented in Table 2 under the column labeled "Net of Market." Decomposing the variance in log earnings growth within markets, we find that 38 percent is due to permanent shocks at the worker level, 57 percent can be attributed to transitory shocks at the worker level, and 2 percent is due to the pass-through of permanent shocks to value added at the firm level. Conditional on the full set of year times market fixed effects, the estimated pass-through rate $\hat{\gamma}$ is 0.13. By comparison, the estimated pass-through rate of permanent market shocks is as large as 0.18. This finding highlights the importance of distinguishing between shocks that are specific to workers in a given firm versus those that common to workers in a market. In Appendix Figure A.3, we control for a full set of year times market fixed effects when analyzing gross household income and net household income, finding also here an important role for shocks that are common to the market.

The second approach we use to examine the variability across regions and industries in income volatility (and its sources) is to estimate the earnings and value added processes separately for each broad market. This estimation generates market-specific parameters for the processes. We perform this analysis separately for earnings, gross income, and net income. The results are presented in Appendix Figure A.4. These results reveal that a pass-through rate of log value added permanent shocks to workers' earnings that is higher in the goods sector as compared to the services sector. However, the Federal tax-and-transfer system attenuates some of these differences.

Robustness checks

Appendix Figure A.2a explores how the pass-through rate varies across worker types by estimating the earnings and value added processes separately for each subgroup. Conditional on a full set of year times market fixed effects, we find that the pass-through estimate does not vary much by the worker's age, previous wage, or gender. Moreover, the pass through rates do not change materially if we restrict the sample to workers who were first hired at the firm in the beginning of the eight year employment spell versus those that have stayed in the firm for a longer time.

In Appendix Figure A.2b, we also present results from several other specification checks. Following Guiso et al. (2005), our main measure of firm performance is value added. They offer two reasons for using value added as a measure of firm performance. First, they argue, value added is the variable that is directly subject to stochastic fluctuations. Second, firms have discretionary power over the reporting of profits in balance sheets, which makes profits a less reliable objective to assess. Nevertheless, it is reassuring to find that the estimates of the pass-through rates are broadly similar if we measure firm performance by operating profits, earnings before interest, tax and depreciation (EBITD), or value added net of reported depreciation of capital. We also show that the estimated pass-through is in the same range as our baseline result if we exclude multinational corporations (for which it can be difficult to accurately measure value added) or exclude the largest firms (which are more likely to have multiple plants).

Our analyses so far have relied on statistical processes of earnings and value added. While this is common, the identification of shocks and pass-through rates relies on plausible but ultimately debatable identifying assumptions. One concern is that individuals may change their behavior in important ways in response to income shocks. For example, an exogenous increase in income could cause workers to decrease labor supply, which could lead us to underestimate the pass-through to workers' earnings of firm shocks. More indirect sources of bias in the estimation of pass-through to workers could arise if individuals respond on margins that are correlated with labor supply and earnings, such as capital investment, expenditure, marriage and fertility decisions, geographic mobility, health investments, and retirement. To assess this, we analyze information in Form W-2G on state lottery winnings. In particular, we leverage variation in the timing of a lottery win, and form cohorts of lottery winners that win in different years. We observe that prior to winning the lottery, the trend in employment, earnings, and other outcomes evolve very similarly across lottery winning cohorts. This suggests a quasi-experimental research design where we use later winners in the years prior to their lottery win as a control group for current winners in the same calendar years. Concretely, this research design amounts to a difference-in-differences analysis, where we use future winners to net out changes in the outcome of interest due to common macroeconomic/time effects as well as common effects of the passage of time, with the underlying identifying assumption that the exact timing of the lottery win is unrelated to pre-existing outcome trends. We develop this identification strategy in greater detail in Appendix D.1.

In Appendix Table A.11 (found in Appendix D.2), we summarize difference-in-differences estimates of the the behavioral responses to exogenous changes in income induced by lottery winnings in terms of earnings, employment, and related behavioral response margins. Reassuringly, we find modest effects on both employment and earnings per dollar won. On average, prize winners reduce their employment by roughly $0.02 per dollar won. After estimating the average effects of income changes induced by lottery winnings, we explore heterogeneity in impacts in various dimensions such as age and prize size. The results are reported in Appendix D.3. Furthermore, Appendix D.4 summarizes related results, but in terms of per-period income, where we use two distinct approaches to allocating one-time lottery income shocks into annual income shocks.

Taken together, our analysis of lottery-induced changes in income lends support to the modeling of the income process. One remaining concern, however, is that the identification of firm shocks and pass-through rates rely on plausible but ultimately debatable identifying assumptions. To critically examine and relax these assumptions – and thereby improve the quality and credibility of our analyses – we extract observable firm shocks based on outcomes of public procurement auctions run by state governments, particularly departments of transportation (DOTs). When two firms bid blindly for the same contract, and one firm happens to win by a small margin, a (quasi)experiment is produced. The winning firm receives an as good as random change in the demand for their products or services, producing exogenous shocks to their output, revenue and labor demand. Using many such experiments from state government DOT public auction data, we examine the salience of these shocks on firm balance sheet outcomes and trace out how these shocks transmit to workers.

We use the experiment of winning a procurement auction to estimate the pass-through of firm-specific shocks. To do so, we match the records of firms that bid in procurement auctions to their tax information using a matching algorithm. Appendix Table A.4(a) demonstrates that the matching algorithm performs well in validation exercises. Appendix Table A.4(b) provides an overview of the sample and shows that it is representative of the broader economy. Nationally, the firms that were matched to procurement auctions represent around 10% of all value added and employment in the construction industry. Appendix Table A.4(c) provides basic sample characteristics on the main outcome variables of interest. Given this data, Appendix Figures

A.5(a-b) visualize the experimental design. These figures show how we compare firms that win a procurement auction for the first time (the treated group) to firms that bid at the same time as the treated group but lose the auction (the control group). Appendix Figure A.5(a) shows how the control group is able to win some auctions in later years and partially catch up to the treated group, while Appendix Figure A.5(b) shows that the treated group continues to win more auctions than the control group in the years after the auction occurs.

Appendix Figures A.5(c-g) demonstrate the salience of winning a procurement auction on firm balance sheet outcomes. These figures show that firms that win procurement auctions immediately see an increase in sales, sales net of procurement winnings, sales net of expenditures (a profit measure), EBITD (another profit measure), and intermediate costs (the cost of goods sold), respectively. These results show that, if a firm wins an auction, it increases sales by 17%, sales net of procurement winnings by 6%, profits by 14%, and expenditure on intermediates by 15%. We see that treatment and control firms are on similar paths prior to auction entry, then treatment firms rapidly respond to winning.

Given that the procurement auctions generate salient observed exogenous shocks to firms, we can now use them to investigate the pass-through of shocks from firms to their workers. Appendix Figures A.5(h-l) provide estimates of the pass-through of firm shocks to the number of workers, the wage bill, and the mean earnings per worker. We find that the number of workers increase by about 10%, the wage bill increases by around 12%, and the earnings per worker increases by around 2%. These effects appear soon after the auction occurs and are persistent. Together, the effects on number of workers and on their earnings suggest an elasticity of labor supply of about 4.1 and a pass-through rate of shocks just below 20%. Reassuringly, these estimates align with our main estimates above based on statistical processes.

To validate the robustness of these results, we consider a number of checks. In Appendix Figure A.5(m), we estimate the pass-through of procurement shocks to stayers who were employed at the firm for two years prior to the auction and remained employed at the firm for two years after, finding a similar effect. In Appendix Figure A.8, we re-estimate these effects on the mean earnings of stayers when varying the stayer length and the years of tenure, finding nearly identifical results. In Appendix Figure A.5(n), we examine the skill composition of new hires using previous earnings as a proxy, finding no evidence that firms hire more skilled workers in response to a demand shock, consistent with an important assumption in our main analysis. In Appendix Figures A.6 and A.7, we vary the definition of the control group in the procurement auctions by requiring that the losing firms were "close" to winning the auction in a cardinal sense (they bid a similar dollar amount) or in an ordinal sense (they were one of the lowest-ranking bidders in the auction), respectively, finding very similar results.

In Appendix Figure A.9, we conclude the procurement auction analysis with four additional robustness checks. First, we restrict the control group to "non-winners", which are firms that not only lose the auction at event time zero but also lose auctions in subsequent years. Second, we restrict to the firms located in "known EIN" states, which are the states for which we were able to use EIN matching rather than name and address matching. Third, we include "all time periods" in the analysis rather than restricting to the time periods around the auction. Fourth, we use a "within" auction specification, in which we compare the treated firms only to the control firms that participated in the same auction as them with fixed effects for each auction. Across all four checks, we find relatively similar results, confirming that our results are robust.

4.2 Mover analyses

So far, we focused on income volatility among workers who stay in the same firm over time. We now turn attention to an approach which examines income changes associated with a worker entering a new firm.

In the baseline results, we consider a special case which assumes that $\phi_{ij} = x_i + \psi_j$ and that $\gamma = \Upsilon = 0$. The first restriction imposes a log additive structure on the earnings that worker i can expect to receive from working in firm j. Under this functional form, the worker fixed effect captures the (time-invariant) portable component of earnings ability, whereas the firm fixed effect can be interpreted as a firm-specific relative pay premium. The second restriction assumes there is no pass through of firm or market level shocks. As a result, the firm effects on earnings do not vary over time. By invoking these two restrictions, our statistical model of earnings reduces to the two-way (worker and firm) fixed effect model of AKM. Appendix C.2 presents results from relaxing these assumptions, and Appendix C.3 provides additional robustness checks.

Under the above restrictions, the variance of log earnings can be written as:

$$Var(\log W_{it}) = \underbrace{Var(x_i + \mathcal{X}_{it}'b)}_{\text{Worker component}} + \underbrace{Var(\psi_{j(i,t)})}_{\text{Firm component}} + \underbrace{2Cov(x_i + \mathcal{X}_{it}'b, \psi_{j(i,t)})}_{\text{Sorting component}} + \underbrace{Var(\epsilon_{it})}_{\text{Residual}} \quad (1)$$

where the worker and firm components tell us how much of the variation in log earnings can be attributed to heterogeneity in worker and firm effects, respectively. The third component captures the contribution to earnings inequality from the sorting of workers to firms. The goal is to quantify these three components to draw inference about the determinants of earnings inequality in the U.S. economy. The decomposition includes both workers who move between firms and stayers. However, the firm and worker effects are only separately identified within a connected set of firms that are linked by worker mobility. Consistent with previous work, we therefore restrict our sample of workers (including stayers and movers) to those who work at a firm in the largest connected set in each time interval (2001-2008 and 2008-2015). In the U.S., this set covers more than 90 percent of the workers (see Appendix Table A.5).

In Table 3, we present results from the variance decomposition in (1) based on data for all firms and workers in the connected set (which includes both workers who move between firms and stayers). This table reports estimates of the worker, firm and sorting components as defined in equation (1). Appendix Table A.6 shows estimates of the subcomponents in the second equality of (1). Consider first Panel A of Table 3 where we present estimates from the AKM estimator for two different time periods (2001-2008 and 2008-2015) as well as pooled estimates where we combine the data from these time periods. The results show that the worker, firm and sorting components change little over time. Therefore, we focus attention on the pooled

Years:		**2001-2008**	**2008-2015**	**Pooled**
Panel A.			**AKM Estimation**	
Share explained by:				
i) Worker Effects	$Var(x_i)$	75%	75%	75%
ii) Firm Effects	$Var(\psi_{j(i)})$	9%	9%	9%
iii) Sorting	$2Cov(x_i, \psi_{j(i)})$	5%	6%	5%
Sorting Correlation:	$Cor(x_i, \psi_{j(i)})$	0.09	0.11	0.10
Panel B.			**BLM Estimation**	
Share explained by:				
i) Worker Effects	$Var(x_i)$	72%	72%	72%
ii) Firm Effects	$Var(\psi_{j(i)})$	3%	3%	3%
iii) Sorting	$2Cov(x_i, \psi_{j(i)})$	13%	14%	14%
Sorting Correlation:	$Cor(x_i, \psi_{j(i)})$	0.43	0.46	0.44

Table 3: AKM and BLM Log Earnings Decomposition Estimates

Notes: This table presents the decomposition of log earnings variation using the AKM and BLM estimators for two time periods.

estimates. These results suggest that the firm effects explain around 9 percent of the variation in log earning, whereas worker sorting accounts for 5 percent. The correlation between firm effects and worker effects is only 0.1.

Next, consider Panel B of Table 3 where we report the BLM estimates. As discussed in Appendix C.1, a possible advantage of the BLM estimator is that it addresses limited mobility bias. Once we correct for such bias we find that firm effects are very small in the U.S. labor market, accounting for only 3 percent of the variation in log earnings. Instead, a larger part of the earnings variation is explained by worker sorting. The correlation between firm effects and worker effects exceeds 0.4 once we correct for limited mobility bias. This finding suggests that sorting of better workers to better firms is an empirically important feature of the U.S. labor market. Detailed sorting patterns are presented in Appendix Figure A.14.

In Table 4, we repeat the AKM and BLM analyses to understand the roles of firm effects, worker effects, and the sorting of workers to firms in explaining gross household income and net household income. We find that moving to a new firm causes even smaller changes in gross and net income as compared to earnings. Moreover, sorting of better workers to better firms contribute less to inequality in gross and net income than to dispersion of earnings. By contrast, worker effects explain a larger part of the variation in gross and net income as compared to gross earnings. Finally, the sorting of workers to firms explains about 5% of the variance in each income measure compared to 9% for earnings, indicating that the Federal tax-and-transfer system attenuates the incentives for better workers to move to better firms.

Income Measure:		Earnings	Gross Income	Net Income
		Panel A. AKM Estimation		
Share explained by:				
i) Worker Effects	$Var(x_i)$	75.4%	83.6%	84.4%
ii) Firm Effects	$Var(\psi_{j(i)})$	8.8%	5.0%	4.7%
iii) Sorting	$2Cov(x_i, \psi_{j(i)})$	4.9%	2.6%	2.2%
Sorting Correlation:	$Cor(x_i, \psi_{j(i)})$	0.09	0.06	0.05
		Panel B. BLM Estimation		
Share explained by:				
i) Worker Effects	$Var(x_i)$	72.4%	81.0%	84.0%
ii) Firm Effects	$Var(\psi_{j(i)})$	3.2%	0.4%	0.4%
iii) Sorting	$2Cov(x_i, \psi_{j(i)})$	12.9%	5.4%	4.8%
Sorting Correlation:	$Cor(x_i, \psi_{j(i)})$	0.43	0.45	0.44

Table 4: AKM vs BLM by Income Measure

Notes: This table presents AKM and BLM decomposition estimates for log earnings, gross income, and net income.

Inequality within and between firms

We now shift attention to describing the inequality within and between firms. To do so, we follow Song et al. (2018) in expressing the variance of log earnings as:

$$\begin{aligned} Var(\log W_{it}) &= \underbrace{Var\left(\log W_{it} - \mathbb{E}\left[\log W_{it}|j(i,t)=j\right]\right)}_{\text{Within}-\text{firm}} + \underbrace{Var\left(\mathbb{E}\left[\log W_{it}|j(i,t)=j\right]\right)}_{\text{Between}-\text{firm}} \\ &= \underbrace{Var\left(x_i + \mathcal{X}_{it}'b - \mathbb{E}\left[x_i + \mathcal{X}_{it}'b|j(i,t)=j\right]\right)}_{\text{Worker heterogeneity within firms}} + \underbrace{Var(\epsilon_{it})}_{\text{Residual}}, \\ &+ \underbrace{Var\left(\psi_{j(i,t)}\right)}_{\text{Firm effects}} + \underbrace{2Cov\left(x_i + \mathcal{X}_{it}'b, \psi_{j(i,t)}\right)}_{\text{Sorting}} + \underbrace{Var\left(\mathbb{E}\left[x_i + \mathcal{X}_{it}'b|j(i,t)=j\right]\right)}_{\text{Segregation}} \end{aligned} \quad (2)$$

where the first equality expresses the variance of log earnings in terms of inequality within and between firms, and the second equality decomposes these terms into economically interpretable subcomponents. Our interest is centered on the last three subcomponents, which capture distinct sources of inequality between firms: dispersion of firm pay premiums ("Firm effects"); sorting of high earning workers into high paying firms ("Sorting"); and worker segregation which reflects differences in the quality of the workforce across firms ("Segregation"). Both worker sorting and segregation reflect non-random allocation of workers to firms. However, sorting matters for aggregate inequality, whereas segregation does not. This is because an increase in segregation will be offset by a reduction in within-firm inequality. Thus, changes in segregation by itself does not affect earnings inequality; it does, however, matter for the relative importance of inequality within versus between firms.

To perform the decomposition in (2), we use exactly the same sample as in Table 3 which includes both workers who move between firms and stayers. The results are presented in Table 5. In Panel A, we report the terms in the first equality. We find that around one-third of the variance of log earnings can be accounted for by the dispersion of average earnings between firms. The remainder is due to heterogeneity across workers within firms. A comparison of the estimates across the two first columns suggests the between firm component has become slightly more important for inequality over time. This finding is broadly consistent with the results reported in Song et al. (2018).[4]

In the next two panels of Table 5, we use the procedures of AKM and BLM to estimate the subcomponents from the second equality. There are three main findings from this analysis. First, a vast majority of the inequality within firms can be accounted for by the observable characteristics and the fixed effects of the workers. Indeed, only 16 percent of the within-firm inequality reflects time-varying unobservables of the worker. Second, once one addresses limited mobility bias then firm effects explain only 10 percent of the inequality between firms. By comparison, sorting of high earning workers to high paying firms accounts for 40 percent while the remaining 50 percent can be attributed to worker segregation that is unrelated to firm pay premiums. Third, there seems to be little if any changes in the relative importance of firm effects, worker sorting and segregation over the time intervals we consider.

Our finding of the inequality contribution from firm effects changing little over time is consistent with Song et al. (2018), albeit their analysis uses AKM and thus suffers from limited mobility bias. Table 5 reveals, however, that this bias does not change materially over the time intervals we consider. As a result, bias correction seems to be empirically important for accurately describing the cross-sectional distribution of earnings in the U.S., but not for understanding the growth in earnings inequality.[5]

Analyses of firm entry and exit

While the analyses discussed above allow us to understand the income volatility due to shocks to or mobility between existing firms, they do not tell us how workers are affected by entry or exit of firms in the same location. Intuitively, when a large factory opens or shuts down, we expect it to impact the commuting zone as a whole rather than only its own workers, affecting important economic outcomes like income variability, tax payments, and the unemployment rate. As explained in Appendix E.1, we obtain an instrumental variable for firm entry and exit with plausibly exogenous variation by analyzing how aggregate fluctuations in foreign economies may affect foreign-owned firms' decisions to enter or exit a location, making use of the information on foreign ownership. In particular, when a foreign economy expands or contracts, we expect it

[4]The analyses in Song et al. (2018) is based on data from 1978 to 2013. Over this longer time period, they show that earnings inequaliy increased considerably, primarly due to a significant rise in the dispersion of average earnings across firms. During the period we consider, however, Song et al. (2018) also report a modest increase in earnings inequality, overall and between firms.

[5]Song et al. (2018) also argue that increases in sorting and segregation caused a large increase in between-firm inequality from 1981 to 2013. At first sight, it would seem like this is inconsistent with our findings. However, most of these increases happen before our data start. During the intervals since 2001 that we consider, Song et al. (2018) report modest increases in the contributions to between-firm inequality from sorting and segregation and a modest decrease from firm effects, consistent with our AKM estimates.

Years:		2001-2008	2008-2015	Pooled
Panel A.		**Total Decomposition**		
Within Firm Share:	$Var(w_{it} - \mathbb{E}[w_{it}\|j])$	67%	64%	66%
Between Firm Share:	$Var(\mathbb{E}[w_{it}\|j])$	33%	36%	34%
Panel B.		**AKM Decomposition**		
Shares of Within Firm Variance:				
Worker Heterogeneity:	$Var(x_i + X_{it}'b - \mathbb{E}[x_i + X_{it}'b\|j])$	84%	85%	84%
Residual:	$Var(\epsilon_{it})$	16%	15%	16%
Shares of Between Firm Variance:				
Firm Effects:	$Var(\psi_j)$	27%	25%	26%
Segregation:	$Var(\mathbb{E}[x_i + X_{it}'b\|j])$	58%	59%	59%
Sorting:	$2Cov(x_i + X_{it}'b, \psi_j)$	15%	16%	15%
Panel C.		**BLM Decomposition**		
Shares of Within Firm Variance:				
Worker Heterogeneity:	$Var(x_i + X_{it}'b - \mathbb{E}[x_i + X_{it}'b\|j])$	83%	84%	84%
Residual:	$Var(\epsilon_{it})$	17%	16%	16%
Shares of Between Firm Variance:				
Firm Effects:	$Var(\psi_j)$	10%	10%	10%
Segregation:	$Var(\mathbb{E}[x_i + X_{it}'b\|j])$	50%	50%	50%
Sorting:	$2Cov(x_i + X_{it}'b, \psi_j)$	40%	40%	40%

Table 5: AKM and BLM Within and Between Inequality Decompositions

Notes: This table presents the decomposition of log earnings variation within and between firms using the AKM and BLM estimators for two time intervals. The analysis uses both workers who move between firms and stayers.

to result in more entry or exit of foreign-owned firms in the commuting zones with higher initial concentration of activity by owners from that country, allowing us to draw causal inference (under plausible identifying assumptions) by comparing those locations that do and do not receive these shocks.

In Appendix E.2, we present and discuss the parameter estimates and the results from a number of specifications and robustness checks. As shown in Appendix Table A.21, we find a positive and statistically significant effect of a firm entering the market on the earnings of workers at existing firms in the same commuting zone. To put the estimates in context, we find that, if a firm employing 10 percent of employees in the commuting zone exits and lays off its workers, then workers in existing employment relationships at other firms in the same commuting zone experience a 4.5% decline in employment, a 4.7% decline in earnings payments to workers, and a 6.4% decline in the firm's value added. Interestingly, as shown in Appendix Table A.25, we find that a layoff shock due to firm exit results in a substantial decrease in gross and net income among workers at other firms in the same commuting zone. However, the impact on net income is smaller than the effect on earnings and gross income, and the estimates imply that the Federal tax-and-transfer system provides about a 9 percent rate of insurance against shocks due to other firms entering and exiting.

4.3 Income volatility, tax revenues, and receipt of tax credits

So far, we have focused on documenting income volatility, quantifying its sources, and examining the attenuation from the Federal tax-and-transfer system. We now shift attention to examining how income volatility, at the individual and market level, may make it difficult to predict tax revenues or receipt of tax credits.

Income processes and mover analyses

The goal is to use the estimates from the income processes and mover analyses to i) examine how various sources of income volatility may generate change in tax revenues, and ii) illustrate how the impact on tax revenues of income volatility may depend on the progressivity of the tax system. As a first step, however, it is convenient to parametrize the tax schedule. Following Heathcote et al. (2014) and Blundell et al. (2016), we choose the following log-linear parametrization to approximate the effective tax rates implicit in the Federal tax-and-transfer system.

$$\tilde{I}_{i,t} = \tau I_{it}^{\lambda}$$

where I denotes gross income and $\tilde{I}$ denotes net income. We estimate these parameters outside the model. In each year, we regress log net household income (earnings plus other income minus taxes) on log household gross income (earnings plus other income) for our sample. The construction of these income measures is detailed in Appendix A. The intercept from this regression gives us τ while λ is identified from the slope coefficient. We estimate τ of around 0.89 whereas λ is estimated to be about 0.92.[6] In a proportional tax-transfer system, λ is equal to one and $(1 - \tau)$ is the proportional effective tax rate. By contrast, if $0 < \lambda < 1$, then the marginal effective tax rate is increasing in earnings. Appendix Figure A.16 shows how well our parsimonious tax function approximates the effective tax rates implicit in the complex U.S. tax-transfer system. Here we compare the predicted log net income from the regression to the observed log net income across the distribution of log gross income, finding that this specification provides an excellent fit.

First, we use the estimated tax schedule to understand how firm shocks are passed-through to tax revenues. To do so, we simulate a one standard deviation shock to log value added at the firm. Then, we use the estimated passthrough rates and the estimated tax-transfer system to collect the implied changes in gross and net income, which in turn provide us the average tax revenue response to a firm shock. Mean tax revenues rise in response to a firm shock in the baseline tax system, as all workers have greater income and marginal tax rates are positive. Finally, we change the parameter λ in order to investigate how the average tax revenue response to the firm shocks depends on tax progressivity. Figure 4 presents the results of this exercise. We find that the responsiveness of tax revenues to firm shocks is greater when the tax schedule is more progressive.

[6] These results mirror closely existing U.S. estimates of τ and λ (see e.g. Guner et al., 2014, Heathcote et al., 2017).

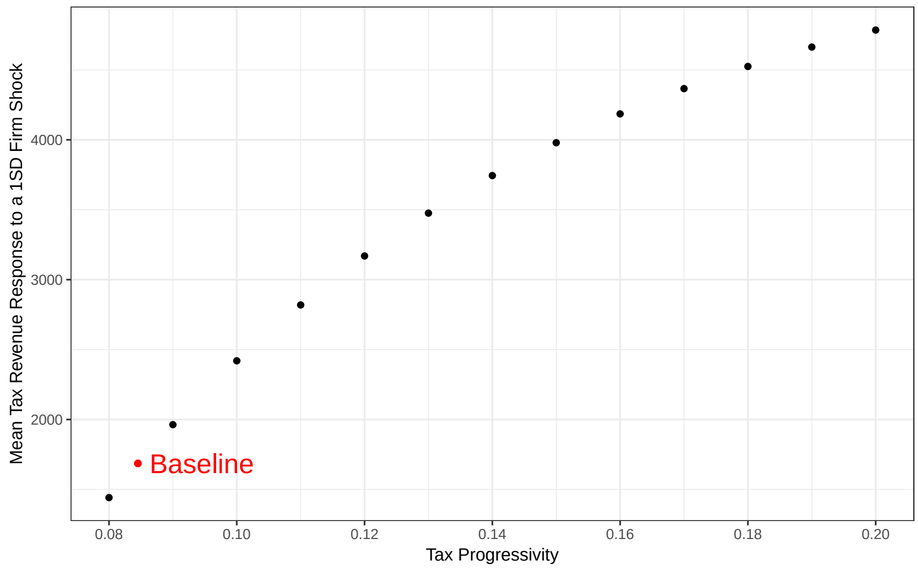


Figure 4: Mean Tax Revenue Responses to a Passed-through Firm Shock, by Tax Progressivity

Notes: In this figure, we consider the effect of a firm shock on mean tax revenues. To do so, we simulate a one standard deviation shock to log value added at the firm. Then, we use the estimated passthrough rates and the estimated tax-transfer system to collect the implied changes in gross and net income, which in turn provide us the average tax revenue response to a firm shock. Finally, we change the parameter λ in order to investigate how the average tax revenue response to the firm shock depends on tax progressivity.

Second, we use the estimated tax schedule to understand how sorting across firms affects mean tax revenues. To do so, we use the AKM model for log gross income (it is the sum of the firm effect, the worker effect, and the worker-year residual) but randomly re-assign firm effects to construct log gross income without sorting. We then use the tax function to collect implied net income and tax revenues for each worker, with and without sorting. Mean tax revenues fall without sorting in the baseline tax system, as fewer workers receive high incomes and high incomes face greater marginal tax rates. Finally, we change the parameter λ in order to investigate how the average tax revenue response to sorting depends on tax progressivity. Figure 5 presents the results of this exercise. We find that the tax revenue gains from sorting are greater when the tax schedule is more progressive.

Analyses of firm entry and exit

Appendix Table A.25 uses the instrumental variables design described in Appendix E.1 in order to estimate the effect that firm entries and exits has on tax payments made by workers employed at other firms in the same commuting zone. It finds a positive and statistically significant effect. To put the estimates in context, we find that, if a firm employing 10 percent of employees in the commuting zone exits and lays off its workers, then workers in existing employment relationships at other firms in the same commuting zone experience a 6.8 percent decline in tax payments.

In order to better understand the responsiveness of tax revenues, we highlight an important

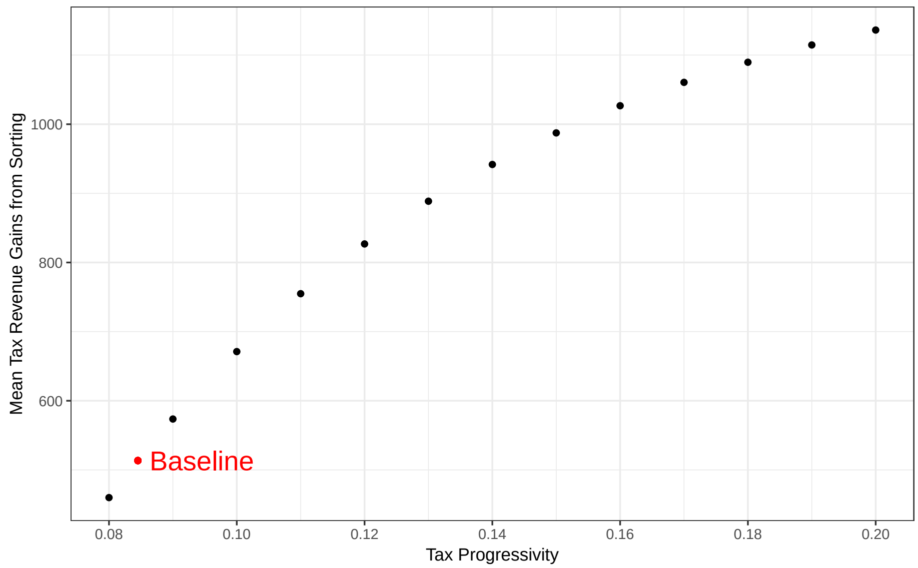


Figure 5: Mean Tax Revenue Gains from Sorting, by Tax Progressivity

Notes: In this figure, we consider the gains from sorting on mean tax revenues. To do so, we randomly re-assign firm effects across workers so that firm effects and worker effects are uncorrelated, then reconstruct log gross income. Then, we use the estimated passthrough rates and the estimated tax-transfer system to collect the implied changes in gross and net income, which in turn provide us the average tax revenue gains from sorting. Finally, we change the parameter λ in order to investigate how the average tax revenue gains from sorting depends on tax progressivity.

component of the system, the Earned Income Tax Credit (EITC). We consider two margins of EITC utilization – claiming any EITC deduction (the extensive margin) and the amount of deduction claimed (the intensive margin).[7] In Appendix Table A.25, we find negative effects of firm entry on both the extensive and intensive margin of EITC participation, though statistical precision is somewhat limited. To put the estimates in context, we find that, if a firm employing 10 percent of employees in the commuting zone exits and lays off its workers, then workers in existing employment relationships at other firms in the same commuting zone experience approximately a 2.4 percent increase in EITC take-up and approximately a 3.1 percent increase in the EITC deduction claimed. These results suggest that the EITC is an active channel through which tax revenues adjust to insure against firm entry and exit shocks for workers employed at other firms in the commuting zone.

[7] Because the EITC is zero either at t or $t-1$ for an observation that experiences a change in the EITC extensive margin, we cannot explore log differences, as we do for other outcomes. Instead, we consider the transformation of Davis et al. (1996), which is able to include any observation that experiences an extensive margin change, and can be interpreted as an approximation to the log difference for small changes; see their paper for further details.

5 Concluding remarks

In this report, we documented income volatility and examined its causes and consequences. Our empirical findings raise questions such as: What do small firm effects, strong sorting and significant pass-through of firm shocks tell us about the functioning of the labor market? How would changes in tax policy affect earnings inequality, worker sorting, income volatility and tax payments? To answer these questions, we have developed an equilibrium model of the labor market that can match the empirical findings that we documented in this report. The model not only allows us to economically interpret the empirical findings reported here but also to improve the analysis of income volatility and taxation. In particular, our model captures that changes in tax policy may induce firms to change their hiring and wage setting, and such changes may affect workers' choice of firm, industry and region. Thus, we can perform model-based simulations of tax reforms which relax the assumption made in Section 4.3 of no behavioral responses of workers and firms to the changes in tax progressivity. See Appendix F for additional results based on the equilibrium model of the labor market.

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A Appendix: Sample Construction and Variable Definitions

All firm-level variables are constructed from annual business tax returns over the years 2001-2015: C-Corporations (Form 1120), S-Corporations (Form 1120-S), and Partnerships (Form 1065). Worker-level variables are constructed from annual tax returns over the years 2001-2015: Direct employees (Form W-2), independent contractors (Form 1099), and household income and taxation (Form 1040).

Variable Definitions:

- **Earnings:** Reported on W-2 box 1 for each Taxpayer Identification Number (TIN). Each TIN is de-identified in our data.

- **Gross Household Income:** Using a definition similar to that of Piketty and Saez (2003), we define gross household income as the sum of taxable wages and other income (line 22 on Form 1040) minus unemployment benefits (line 19 on Form 1040) minus taxable Social Security benefits (line 20a on Form 1040) plus tax-exempt interest income (line 8b on Form 1040). We at times also consider this measure when subtracting off Schedule D capital gains (line 13 on Form 1040).

- **Federal Taxes on Household Income:** This is given by the sum of two components. The first component is the sum of FICA Social Security taxes (given by 0.0620 times the minimum of the Social Security taxable earnings threshold, which varies by year, and taxable FICA earnings, which are reported on Box 3 of Form W-2) and FICA Medicare taxes (given by 0.0145 times Medicare earnings, which are reported on Box 5 of Form W-2). The second component is the sum of the amount of taxes owed (the difference between line 63 and line 74 on Form 1040, which is negative to indicate a refund) and the taxes already paid or withheld (the sum of lines 64, 65, 70, and 71 on Form 1040).

- **Net Household Income:** We construct a measure of net household income as Gross Household Income minus Federal Taxes on Household Income plus two types of benefits: unemployment benefits (line 19 of Form 1040) and Social Security benefits (line 20a of Form 1040).

- **Employer:** The Employer Identification Number (EIN) reported on W-2 for a given TIN. Each EIN is de-identified in our data.

- **Wage Bill:** Sum of Earnings for a given EIN plus the sum of 1099-MISC, box 7 nonemployee compensation for a given EIN in year t.

- **Size:** Number of FTE workers matched to an EIN in year t.

- **NAICS Code:** The NAICS code is reported on line 21 on Schedule K of Form 1120 for C-corporations, line 2a Schedule B of Form 1120S for S-corporations, and Box A of form 1065 for partnerships. We consider the first three digits to be the industry. We code invalid industries as missing.

- **Commuting Zone:** This is formed by mapping the ZIP code from the business filing address of the EIN on Form 1120, 1120S, or 1065 to its commuting zone.

- **Value Added:** Line 3 of Form 1120 for C-Corporations, Form 1120S for S-Corporations, and Form 1065 for partnerships. Line 3 is the difference between Revenues, reported on Line 1c, and the Cost of Goods Sold, reported on Line 2. We replace non-positive value added with missing values.

  – For manufacturers (NAICS Codes beginning 31, 32, or 33) and miners (NAICS Codes beginning 212), Line 3 is equal to Value Added minus Production Wages, defined as wage compensation for workers directly involved in the production process, per Schedule A, Line 3 instructions. If we had access to data from Form 1125-A, Line 3, we could directly add back in these production wages to recover value added. Without 1125-A, Line 3, we construct a measure of Production Wages as the difference between the Wage Bill and the Firm-reported Taxable Labor Compensation, defined below, as these differ conceptually only due to the inclusion of production wages in the Wage Bill.

- **Value Added Net of Depreciation:** Value Added minus Depreciation, where Depreciation is reported on Line 20 on Form 1120 for C-corporations, Line 14 on Form 1120S for S-corporations, and Line 16c on Form 1065 for partnerships.

- **EBITD:** We follow Kline et al. (2018a) in defining Earnings Before Interest, Taxes, and Depreciation (EBITD) as the difference between total income and total deductions other than interest and depreciation. Total income is reported on Line 11 on Form 1120 for C-corporations, Line 1c on Form 1120S for S-corporations, and Line 1c on Form 1065 for Partnerships. Total deductions other than interest and depreciation are computed as Line 27 minus Lines 18 and 20 on Form 1120 for C-corporations, Line 20 minus Lines 13 and 14 on Firm 1120S for S-corporations, and Line 21 minus Lines 15 and 16c on Form 1065 for partnerships.

- **Operating Profits:** We follow Kline et al. (2018a), who use a similar approach to Yagan (2015), in defining Operating Profits as the sum of Lines 1c, 18, and 20, minus the sum of Lines 2 and 27 on Form 1120 for C-corporations,, the sum of Lines 1c, 13, and 15, minus the sum of Lines 2 and 20 on Form 1120S for S-corporations, and the sum of Lines 1c, 16, and 16c, minus the sum of Lines 2 and 21 on Form 1065 for partnerships.

- **Firm-reported Taxable Labor Compensation:** This is the sum of compensation of officers and salaries and wages, reported on Lines 12 and 13 on Form 1120 for C-corporations, Lines 7 and 8 on Form 1120S for S-corporations, and Lines 9 and 10 on Form 1065 for Partnerships.

- **Firm-reported Non-taxable Labor Compensation:** This is the sum of employer pension and employee benefit program contributions, reported on Lines 17 and 18 on Form 1120 for C-corporations, Lines 17 and 18 on form 1120S for S-corporations, and Lines 18 and 19 on Form 1065 for Partnerships.

- **Multinational Firm:** We define an EIN as a multinational in year t if it reports a non-zero foreign tax credit on Schedule J, Part I, Line 5a of Form 1120 or Form 1118, Schedule B, Part III, Line 6 of Form 1118 for a C-corporation in year t, or if it reports a positive Total Foreign Taxes Amount on Schedule K, Line 16l of of Form 1065 for a partnership in year t.

- **Foreign Ownership:** We define an EIN as foreign-owned in year t if it files Form 5472 in year t. The country of foreign ownership is also reported on Form 5472.

- **Tenure:** For a given TIN, we define tenure at the EIN as the number of prior years in which the EIN was the highest-paying. A TIN has No Tenure if Tenure is zero years and has High Tenure if tenure is at least five years.

- **Age and Sex:** Age at t is the difference between t and birth year reported on Data Master-1 (DM-1) from the Social Security Administration, and sex is the gender reported on DM-1 (see Chetty et al. (2011) for further details on the DM-1 link). We define Young Age as Age less than or equal to 45 years, and Not Young Age as Age greater than 45 years.

- **Gross State Lottery Winnings**: This is the total reported in Box 1 of Form W-2G when the form is identified as a state lottery payment in Box 3 of Form W-2G.

- **Adjusted Gross Income**: This is the tax-payer unit (TPU) adjusted gross income reported on Form 1040, divided by 2 in households of married filers.

- **Observed Capital Income:** This is the tax-payer unit (TPU) total observed capital income (excluding capital gains), defined as the sum of dividends, interest income, pension income, rent and royalty income, and miscellaneous Schedule E rental income. All components are reported on Form 1040. We divide by 2 in households of married filers.

- **Marginal Tax Rate:** This is the tax-payer unit (TPU) change in total taxes owed (state + federal) for a $1 change in TPU wage earnings. We calculate this using a tax calculator written by Jon Bakija.

- **Total Taxes Owed:** This is the tax-payer unit (TPU) total taxes owed (state + federal). We calculate this using a tax calculator written by Jon Bakija.

Sample Definitions:

- **Analysis Sample:** A TIN belongs to the Analysis Sample in year t if (a) her highest-paying EIN on form W-2 has positive Value Added in year t, (b) associated Earnings are at least \$15,000 in year t, (c) the commuting zone and 3-digit NAICS code of the EIN are valid in year t, and (d) the TIN is matched to SSA records and the age associated with the TIN at t is between 25 and 60.

- **Stayers Sample:** A worker belongs to the Stayers Sample in year t if (a) the worker belongs to the Analysis Sample in years t, t-1, . . . , t-7, (b) her associated highest-paying EIN is the same in years t, t-1, . . . , t-7, (c) the commuting zone and industry associated with her highest-paying EIN are the same in years t, t-1, . . . , t-7, (d) there are at least 10 stayers per firm, and (e) there are at least 10 firms per commuting zone and industry.

- **Movers Sample:** A worker belongs to the Movers Sample if (a) the worker belongs to the Analysis sample in years t and s, (b) her associated highest-paying EIN is different in years t and s, and (c) there are at least two movers associated with the EIN.

- **State Lottery Sample:** A worker belongs to the State Lottery Sample in year t if (a) she received a state lottery payment on Form W-2G between 2001 and 2016, (b) the worker is not missing age or sex data from SSA records, (c) she was 21 to 64 years old at the time of receiving the Form W-2G, and (d) her first recorded W-2G state lottery payment between 2001 and 2016 was for \$30,000 or more.

- **Procurement Auctions Sample:** A firm belongs to the Procurement Auctions Sample if the matching algorithm detects a string match on name and address. The algorithm, which uses a "fuzzy matching" approach with match quality measured using a sieve, is validated using a subsample of 5 states which provided the firm's EIN in the procurement auction records and thus permit exact matching. Table A.4(a) shows that the algorithm outperforms a simple text search (85% versus 79%).

	Goods				Services				All
	Midwest	Northeast	South	West	Midwest	Northeast	South	West	All
Panel A.	**Full Sample**								
Observation Counts:									
Number of FTE Worker-Years	42,910,324	26,701,886	40,332,913	31,598,149	69,049,669	62,399,969	103,263,800	71,385,819	447,642,529
Number of Unique FTE Workers	9,319,084	6,088,816	10,218,947	7,714,829	17,315,144	15,168,284	26,530,182	17,953,911	89,579,704
Number of Unique Firms with FTE Workers	294,907	232,740	439,823	329,721	1,051,608	1,055,084	1,908,800	1,314,677	6,479,326
Number of Unique Markets with FTE Workers	1,514	270	1,780	916	4,108	761	4,926	2,509	16,164
Group Counts:									
Mean Number of FTE Workers per Firm	22.1	17.8	16.1	16.3	10.4	9.7	9.5	9.6	11.4
Mean Number of FTE Workers per Market	2,007.0	6,778.8	1,581.7	2,524.2	1,217.4	5,623.1	1,488.4	2,084.0	1,906.6
Mean Number of Firms per Market with FTE Workers	91.0	380.6	98.0	155.2	117.0	577.9	156.2	216.3	166.9
Outcome Variables in Log $:									
Mean Log Wage for FTE Workers	10.76	10.81	10.70	10.81	10.61	10.74	10.62	10.70	10.69
Mean Value Added for FTE Workers	17.36	16.80	16.67	16.64	16.18	16.04	15.94	16.07	16.31
Firm Aggregates in $1,000:									
Wage Bill per Worker	43.6	50.7	42.2	52.9	34.3	44.2	35.8	40.3	40.9
Value Added per Worker	91.2	107.5	85.1	91.6	90.5	111.1	94.2	92.3	95.2
Panel B.	**Movers Sample**								
Observation Counts:									
Number of FTE Mover-Years	17,458,234	11,545,098	18,078,675	15,521,491	31,647,628	28,398,961	50,074,776	35,344,937	208,069,800
Number of Unique FTE Movers	4,125,425	2,830,268	4,822,238	3,877,827	7,724,643	6,663,264	11,909,494	8,324,587	32,077,850
Number of Unique Firms with FTE Movers	188,405	144,294	265,504	215,212	571,413	549,162	1,019,393	700,921	3,560,534
Number of Unique Markets with FTE Movers	1,463	266	1,753	878	3,915	755	4,783	2,359	15,609
Group Counts:									
Mean Number of FTE Movers per Firm with FTE Movers	13.5	11.9	11.2	11.6	8.2	7.9	7.9	8.2	8.9
Mean Number of Movers per Market with FTE Movers	862.4	2,964.1	730.3	1,310.7	597.7	2,617.4	759.3	1,116.4	936.7
Mean Number of Firms per Market with FTE Movers	64.0	248.9	65.3	112.8	72.6	332.3	96.1	136.8	105.0
Outcome Variables in Log $:									
Mean Log Wage for FTE Movers	10.76	10.81	10.70	10.81	10.61	10.74	10.62	10.70	10.69
Mean Value Added for FTE Movers	17.36	16.80	16.67	16.64	16.18	16.04	15.94	16.07	16.31
Panel C.	**Stayers Sample**								
Sample Counts:									
Number of 8-year Worker-Firm Stayer Spells	2,588,628	1,777,928	1,237,821	1,150,115	2,315,238	2,527,212	2,609,997	2,207,552	16,506,865
Number of Unique FTE Stayers in Firms with 10 FTE Stayers	798,575	532,507	416,549	354,518	740,091	764,699	865,629	724,155	5,217,960
Number of Unique Firms with 10 FTE Stayers	13,884	10,896	9,409	9,767	18,083	19,475	19,626	16,185	117,698
Number of Unique Markets with 10 Firms with 10 FTE Stayers	197	111	216	104	335	213	438	219	1,826
Outcome Variables in Log $:									
Mean Log Wage for FTE Stayers	10.95	10.99	10.97	10.99	10.90	11.01	10.96	11.05	10.97
Mean Log Value Added for FTE Stayers	18.04	17.56	17.46	16.56	17.45	17.23	17.89	17.93	17.61

Table A.1: Detailed sample characteristics

Notes: This table provides a detailed examination of the full sample, movers sample, and stayers sample.

B Appendix: Earnings process, value added process and pass-through

B.1 Explanation of the Empirical Approach

In this section, we use the panel data on workers and firms to describe key features of the U.S. labor market. We begin by describing the statistical model of earnings that we will apply to this data. Next, we present the empirical findings, and then discuss how they motivate and guide our choices of how to model the labor market.

B.1.1 Statistical model of earnings

We assume that workers' earnings can be described by the following equation:

$$\log W_{it} = \mathcal{X}_{it}'\vartheta + w_{it}, \tag{3}$$

where W_{it} denotes the earnings for individual i in year t, $\mathcal{X}_{it}$ is a vector of covariates which includes a full set of indicators for calendar years and a cubic polynomial in age, and w_{it} denotes log earnings net of age effects and common aggregate time trends. As described below, we allow w_{it} to depend on both the workers' own productivity and the firm in which she works. Our measure of firm performance is value added, which is determined by the equation:

$$\log Y_{jt} = \mathcal{Z}_t' \varphi + y_{jt}, \tag{4}$$

where Y_{jt} denotes the value added for firm j in year t, $\mathcal{Z}_t$ includes a full set of indicators for calendar years, and y_{jt} is log value added net of common aggregate time trends. The key elements of equations (3) and (4) are the time series properties of w_{it} and y_{jt}, which we now specify.

Specification of processes We assume that y_{jt} evolve according to the following process:

$$\begin{aligned} y_{jt} &= \zeta_j + y^p_{jt} + \xi_{jt} + \delta^y \xi_{jt-1} \\ y^p_{jt} &= y^p_{jt-1} + u_{jt}, \\ u_{jt} &= \tilde{u}_{jt} + \bar{u}_{r(j),t} \end{aligned} \tag{5}$$

where $r(j)$ denotes the market of firm j, ζ_j is a fixed effect for the firm, and the time-varying part of y_{jt} is decomposed into a permanent component, assumed to follow a unit root process with innovation shock u_{jt}, and a transitory component, which is assumed to follow a MA(1) process with coefficients δ^y and innovation variance σ^2_ξ. The permanent innovation u_{jt} consists of a common innovation to all firms in a given market r, $\bar{u}_{r(j),t} \equiv \mathbb{E}\left[u_{jt} | r(j){=}r\right]$, and an idiosyncratic innovation specific to the firm, $\tilde{u}_{jt} \equiv u_{jt} - \bar{u}_{r(j),t}$.

We assume that w_{it} evolve according to the following process:

$$\begin{aligned} w_{it} &= \phi_{ij(i,t)} + w^p_{it} + \nu_{it} + \delta^w \nu_{it-1} \\ w^p_{it} &= w^p_{it-1} + \gamma \tilde{u}_{j(i,t),t} + \Upsilon \bar{u}_{r(i,t),t} + \mu_{it}, \end{aligned} \tag{6}$$

where $j(i,t)$ and $r(i,t)$ denote the firm and market of worker i in year t, and ϕ_{ij} is a fixed effect for worker i if she works in firm j. The time-varying part of w_{it} is decomposed into a permanent component w^p_{it} and a transitory component, assumed to follow a MA(1) process with coefficients δ^w and innovation variance σ^2_v. The permanent earnings component evolves for three reasons: worker-specific innovations μ_{it}, pass through of firm-specific value added shocks $\gamma \tilde{u}_{j(i),t,t}$, and pass through of market level value added shocks $\Upsilon \bar{u}_{r(i,t),t}$.

Parameters of interest and assumptions Our interest is centered on two aspects of this statistical model of earnings. The first is how changes in firm performance affect the earnings of incumbent workers, as measured by the pass-through rates γ and Υ. The second is the determinants of the cross-sectional distribution of earnings, which we measure by decomposing ϕ_{ij} into components that capture worker heterogeneity, firm-specific wage premiums, worker sorting, and interactions between worker and firm effects.

For these purposes, it is necessary to invoke some restrictions on the statistical model of earnings. Let $J = \{j(i,t)\}_{i,t}$ and $U = \{\tilde{u}_{jt}, \bar{u}_{r(j),t}\}_{j,t}$ and $Q = \{\xi_{jt}\}_{j,t}$. We make the following assumptions:

Assumption 1. $\mathbb{E}\left[\xi_{jt}|r(j){=}r, J, U\right] = \mathbb{E}\left[\xi_{jt'}\xi_{jt}|r(j){=}r, J, U\right] = 0$ *for all* j, r, t, t'.

Assumption 2. $\mathbb{E}\left[\mu_{it}, \nu_{it}|J, U, Q\right] = 0$ *for all* i, t.

Assumption 1 is the same restriction on the error structure of the value added process as in Guiso et al. (2005). It implies that transitory shocks to value added are mean zero and uncorrelated with past transitory shocks to value added. Assumption 2 is a condition on the relationship between the worker-specific innovations to earnings, worker mobility, and innovations to firm value added. The assumption embodies two types of economic restrictions. The first restriction, from conditioning on $j(i,t)$, implies that mobility is exogenous to the worker-specific innovations to earnings (which are paid to the worker independent of the choice of firm). This is the same restriction on worker mobility as invoked in the Abowd et al. (1999) model. The second restriction, from the conditioning on the innovations to firm value added, implies that the worker-specific innovations to earnings neither co-vary across coworkers nor with shocks to firm value added. This is the same restriction as in Guiso et al. (2005).

It is important to observe what is *not* being restricted under Assumptions 1 and 2. First, we do not restrict whether or how workers sort into firms according to the worker effects, the firm effects, or the interactions between the worker and firm effects. Second, we do not restrict whether or what type of workers move across firms in response to innovations to firm value added. In fact, workers with different values of ϕ_{ij} may have arbitrarily different mobility patterns. Third, the statistical model of earnings does not specify why individuals choose the firm that they do. However, it also does not preclude the possibility that individuals choose firms to maximize earnings or utilities. For instance, Assumptions 1 and 2 are consistent with each worker choosing the firm that offers his preferred combination of wages and non-wage attributes.

B.1.2 Pass through of firm shocks

In this section, we are interested in estimating the parameters γ and Υ, which we refer to as the *pass-through rates* of firm-specific and market level value added shocks. Before presenting estimates of the pass-through rates, we show how these parameters can be identified through a difference-in-differences (DiD) strategy.

Identification, moment conditions and DiD representation To compare with existing work, we first consider a special case of the statistical model of earnings where $\gamma = \Upsilon$. That is, we assume the pass-through rate of an idiosyncratic value added shock to the current firm is of the same size as the pass-through rate of a value added shock to all firms in the current market. We focus on the sample of stayers as captured by the indicator variable $S_i = 1[j(i,1){=}...{=}j(i,T)]$.

Assumptions 1 and 2 give the following moment conditions:

$$\mathbb{E}\left[\Delta y_{j(i)t}\left(w_{it+\tau} - w_{it-\tau'} - \gamma\left(y_{j(i),t+\tau} - y_{j(i),t-\tau'}\right)\right)|S_i{=}1\right] = 0 \quad (7)$$
$$\text{for } \tau \geq 2, \tau' \geq 3$$

Solving for γ we identify the pass through of a firm-specific shock to the earnings of incumbent workers:

$$\gamma = \frac{\mathbb{E}\left[\Delta y_{j(i)t}\left(w_{it+\tau} - w_{it-\tau'}\right)|S_i{=}1\right]}{\mathbb{E}\left[\Delta y_{j(i)t}\left(y_{j(i),t+\tau} - y_{j(i),t-\tau'}\right)|S_i{=}1\right]}$$

Thus, we can identify the pass through of a firm-specific shock from our panel data on firms and workers.

DiD interpretation

To interpret this identification result and assess the underlying assumptions, note that the statistical model of earnings includes fixed effects for time and agents. By controlling for these fixed effects we obtain a DiD strategy, looking within workers and firms while eliminating common changes over time in the labor market or the economy more generally. To see the DiD representation, suppose for simplicity the workers can be assigned to two groups of firms: one half has $\Delta y_{j(i)t} = +\delta$ and the other half has $\Delta y_{j(i)t} = -\delta$. We then get the following interpretation of γ as the ratio of two DiDs.

$$\gamma = \frac{\mathbb{E}\left[w_{it+\tau} - w_{it-\tau'}|{+\delta}, S_i{=}1\right] - \mathbb{E}\left[w_{it+\tau} - w_{it-\tau'}|{-\delta}, S_i{=}1\right]}{\mathbb{E}\left[y_{j(i),t+\tau} - y_{j(i),t-\tau'}|{+\delta}, S_i{=}1\right] - \mathbb{E}\left[y_{j(i),t+\tau} - y_{j(i),t-\tau'}|{-\delta}, S_i{=}1\right]}$$

Under an assumption of common underlying trends between the two groups, the numerator gives the treatment effect on log earnings; the denominator gives the treatment effect on log value added; and the ratio gives the elasticity of earnings with respect to value added.

Graphical evidence

In Figure A.1, we empirically assess the DiD strategy. The figure is constructed in the following way: In any given calendar year (denoted period $t = 0$), we i) order firms according to the increase $\Delta y_{j(i)t}$; ii) separate the firms at the median in the distribution of $\Delta y_{j(i)t}$, letting the upper half constitute the treatment firms and the lower half the control firms; and iii) plot the differences in y_{jt} between these two groups in period $t = 0$ as well as in the years before (periods $t < 0$) and after (periods $t > 0$). We perform these three steps separately for various calendar years, always weighting each firm by the number of workers. The solid (dashed) black line represents the difference in log value added (wages) for the treatment and control firms where each firm is weighted by the number of workers.

By construction, the treatment and control groups differ in the value added growth from period $t-1$ to period t. On average, firms in the treatment group experience about 30 percentage points larger growth in value added as compared to firms in the control group. According to the value added process (5), the growth in value added should be the sum of a permanent component and a transitory, mean-reverting component. Due to the transitory component, $\Delta y_{j(i)t}$ could be correlated with $\Delta y_{j(i)\tau}$ at $\tau = t-2, ..., t+2$. However, $\Delta y_{j(i)t}$ should be orthogonal to $\Delta y_{j(i)\tau}$ in the periods before $\tau = t-2$ and after $\tau = t+2$. Consistent with this orthogonality condition, the figure shows a very similar trend in log value added between the treatment and control group at these periods. Reassuringly, firms that experienced large growth in value added in period 0 are no more or less likely to experience large growth in value added in periods -6 to -3 or in periods 3 to 6.

The dashed black line performs the same exercise, but this time for log wages of incumbent workers who stay in the firm in all six years. On average, workers in treatment firms experience an additional 5 percentage points increase in earnings in period 0 as compared to workers in the control firms. Interpreted through the lens of the DiD design, this finding suggests a pass-through rate of firm shocks γ above .15. The growth in earnings is also the sum of a permanent component and a transitory, mean-reverting component. Therefore, Δw_{it} could be correlated with $\Delta w_{i\tau}$ at $\tau = t-2, ..., t+2$, but it should be orthogonal to $\Delta w_{i\tau}$ in the periods before $\tau = t-2$ and after $\tau = t+2$. Reassuringly, the dashed line shows a very similar trend in log earnings between workers in the treatment and control group during these periods.

Firm versus market level shocks

We now shift attention to the general case where γ may differ from Υ, thereby allowing the earnings of an incumbent worker to respond differently to an idiosyncratic value added shock to the current firm than to a (same size) shock to all firms in a given market. To identify the firm-level pass-through rate γ, we then need to demean the variables of interest using a within market times year transformation, $\tilde{w}_{it} = w_{it} - \mathbb{E}\left[w_{it}|r(i,t)=r\right]$ and $\tilde{y}_{jt} = y_{jt} - \mathbb{E}\left[y_{jt}|r(j)=r\right]$. Assumptions 1 and 2 then give the following moment conditions that we can use to identify the pass-through rates of firm-specific value and market level added shocks by solving for γ and Υ :

$$\mathbb{E}\left[\Delta\tilde{y}_{j(i),t}\left(\tilde{w}_{it+\tau} - \tilde{w}_{it-\tau'} - \gamma\left(\tilde{y}_{j(i),t+\tau} - \tilde{y}_{j(i),t-\tau'}\right)\right)|S_i{=}1\right] = 0 \tag{8}$$

$$\mathbb{E}\left[\Delta\bar{y}_{j(i),t}\left(\bar{w}_{it+\tau} - \bar{w}_{it-\tau'} - \Upsilon\left(\bar{y}_{j(i),t+\tau} - \bar{y}_{j(i),t-\tau'}\right)\right)|S_i{=}1\right] = 0 \tag{9}$$

$$\text{for } \tau \geq 2, \tau' \geq 3$$

where $\bar{y}_{r(j),t} \equiv \mathbb{E}\left[y_{jt}|r(j)=r\right]$ and $\bar{w}_{r(j),t} \equiv \mathbb{E}\left[w_{jt}|r(j)=r\right]$.

The red and blue lines in Figure A.1 represent the differences between the treatment and control group in $(\tilde{w}_{it}, \tilde{y}_{jt})$ and $(\bar{w}_{r(i,t),t}, \bar{y}_{r(j),t})$ over time. These lines are constructed in the same way as the black lines, except the red and blue lines use the demeaned variables $\tilde{w}$ and $\tilde{y}$ and the market averages $\bar{w}$ and $\bar{y}$, respectively. Comparing the red solid line to the red dashed line reveals that conditioning on the full set of year times market fixed effects attenuates slightly the treatment effect on log earnings relative to the treatment effect on log value added. Interpreted through the lens of the DiD design, this finding suggests the estimated pass-through rate of a firm-specific shock will be slightly lower once we allow for Υ to differ from γ. By way of comparison, the DiD applied to the market averages of wages and value added suggests a relatively large estimate of Υ. Thus, we expect the estimated pass-through rate of an idiosyncratic value added shock to the current firm γ to be smaller than the pass-through rate of a same size shock to all firms in the market Υ.

B.2 Tables and Figures

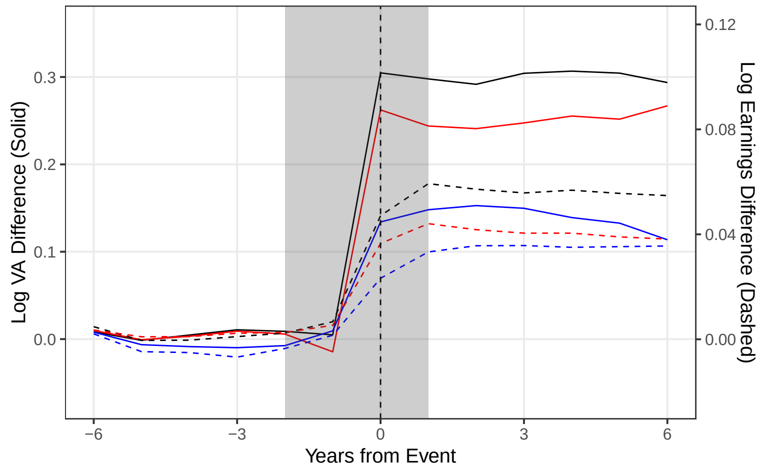


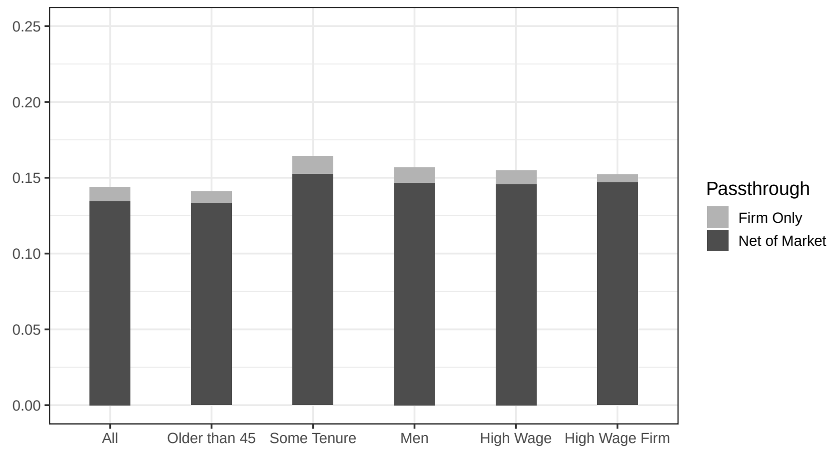
Figure A.1: Differences-in-differences representation of the estimation strategy

Notes: This figure displays the mean differences in log value added (solid lines) and log earnings (dotted lines) between firms that receive an above-median versus below-median log value added change at event time zero. Results are presented for the unconditional measures of log value added and log earnings (black lines), for the measures of log value added and log earnings net of market interacted with year effects (red lines), and for the averages of log value added and log earnings by market and year (blue lines) The shaded area denotes the time periods during which the orthogonality condition need not hold in the identification of the permanent pass-through rate.

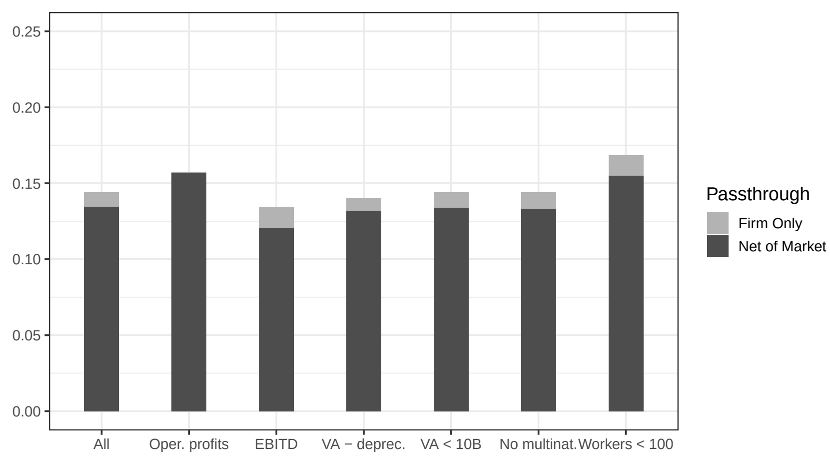
	GMM Estimates of Joint Process			
	Firm Only		Accounting for Markets	
	Log Value Added	Log Earnings	Log Value Added	Log Earnings
Panel A.	**Process: MA(1)**			
Total Growth (Std. Dev.)	0.31	0.17	0.29	0.16
	(0.01)	(0.00)	(0.01)	(0.00)
Permanent Shock (Std. Dev.)	0.20	0.10	0.17	0.10
	(0.01)	(0.00)	(0.01)	(0.00)
Transitory Shock (Std. Dev.)	0.18	0.10	0.17	0.10
	(0.01)	(0.00)	(0.01)	(0.00)
MA Coefficient, Lag 1	0.09	0.15	0.09	0.15
	(0.01)	(0.00)	(0.01)	(0.00)
MA Coefficient, Lag 2	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Permanent Passthrough Coefficient		0.14		0.13
		(0.01)		(0.01)
Transitory Passthrough Coefficient		-0.01		0.00
		(0.01)		(0.00)
Market Passthrough Coefficient				0.18
				(0.02)
Panel B.	**Process: MA(2)**			
Total Growth (Std. Dev.)	0.31	0.17	0.29	0.16
	(0.01)	(0.00)	(0.01)	(0.00)
Permanent Shock (Std. Dev.)	0.20	0.10	0.17	0.10
	(0.01)	(0.00)	(0.00)	(0.00)
Transitory Shock (Std. Dev.)	0.17	0.10	0.17	0.10
	(0.01)	(0.00)	(0.01)	(0.00)
MA Coefficient, Lag 1	0.05	0.21	0.07	0.21
	(0.05)	(0.01)	(0.04)	(0.01)
MA Coefficient, Lag 2	-0.03	0.04	-0.01	0.04
	(0.03)	(0.00)	(0.02)	(0.00)
Permanent Passthrough Coefficient		0.15		0.13
		(0.01)		(0.01)
Transitory Passthrough Coefficient		-0.02		0.00
		(0.01)		(0.00)
Market Passthrough Coefficient				0.18
				(0.03)

Table A.2: Estimated Process for Log Earnings and Pass-through

Notes: This table displays the parameter estimates of the log value added and log earnings growth processes as well as the passthrough coefficients when using the GMM estimator. It presents these estimates for the firm only model (which imposes $\Upsilon = \gamma$) as well as the model in which firm and market pass-through coefficients are allowed to differ (which permits $\Upsilon \neq \gamma$).



(a) Heterogeneity across Workers



(b) Heterogeneity across Firms and V.A. Measures

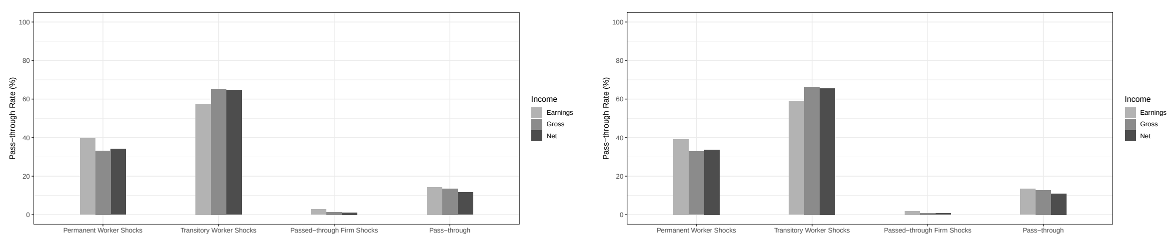
Figure A.2: Pass-through Heterogeneity

Notes: This figure displays heterogeneity in the GMM estimates of the pass-through, both firm only (imposing $\Upsilon = \gamma$) and removing market by year means (permitting $\Upsilon \neq \gamma$).

	Goods				Services			
	Midwest	Northeast	South	West	Midwest	Northeast	South	West
Log Earnings:								
Unconditional	0.156	0.126	0.143	0.176	0.136	0.113	0.143	0.146
Net of Market	0.156	0.132	0.136	0.168	0.124	0.110	0.140	0.119
Market	0.157	0.101	0.163	0.211	0.205	0.139	0.154	0.264
Selection Coefficient	0.250	0.190	0.243	0.184	0.143	0.115	0.238	0.182
Log Size:								
Unconditional	0.454	0.480	0.348	0.441	0.501	0.354	0.479	0.464
Net of Market	0.492	0.517	0.352	0.455	0.480	0.360	0.585	0.444
Other Moments:								
Market Size (millions)	42.9	26.7	40.3	31.6	69.0	62.4	103.2	71.4
Labor Share	0.630	0.663	0.746	0.790	0.660	0.659	0.700	0.662
Profits per FTE Worker ($1,000)	47.6	56.9	43.0	38.7	56.5	66.9	58.4	52.0
Wagebill per FTE Worker ($1,000)	43.6	50.7	42.2	52.9	34.1	44.2	35.8	40.3

Table A.3: Detailed Passthrough Estimates and Aggregate Statistics across Regions and Sectors

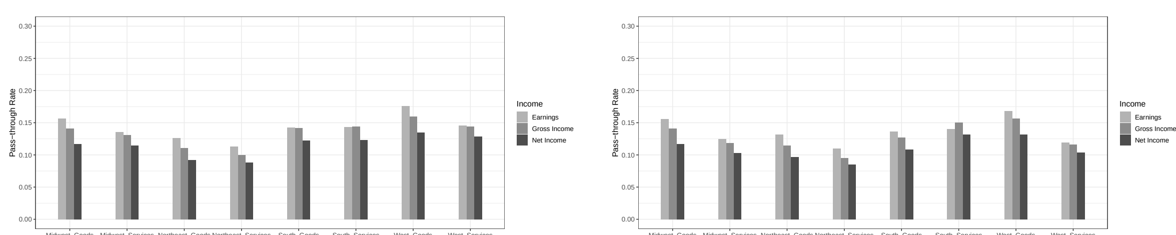
Notes: In this table, we present passthrough estimates for various outcomes as well as other empirical moments across broad regions and sectors.



(a) Firm Only (b) Net of Market

Figure A.3: Growth and Pass-through Estimation for Alternate Income Concepts

Notes: In subfigure (a), we present the shares of (i) earnings, (ii) gross income, and (iii) net income growth attributable to permanent and transitory shocks to workers and permanent shocks to firms, as well as the passthrough rate from firms to workers, in the baseline specification ("Firm Only"). In subfigure (b), we repeat this exercise when conditioning on a full set of market-year indicators ("Net of Market").



(a) Firm Only (b) Net of Market

Figure A.4: Broad Market Heterogeneity in Passthrough Estimation by Income Measure

Notes: In this figure, we present broad market heterogeneity in the firm only and net passthrough rate of permanent shocks from firms to workers.

	Simple Search	Fuzzy Match			
	(1)	(2)	(3)	(4)	(5)
% Bidders Matched to Any Tax Record	80.2	99.9	97.6	99.9	95.8
% Bidders Matched to the True Tax Record	65.3	63.0	62.5	71.0	70.3
% Potential Matches Correctly Matched to Tax Records	78.6	75.8	75.1	85.4	84.5
Algorithm Parameters:					
Match must be perfect (string score = 1.0)	✓	✗	✗	✗	✗
Match must be high-quality (string score ≥ 0.6)	✗	✗	✓	✗	✓
Prefer matches in same state as auction	✓	✗	✗	✓	✓

(a) Algorithm Match Performance

	DOT Auction Records		Final Sample: Matched Auction-Tax Data		
State	**Data Source**	**Includes EIN**	**Bidders in 2010 (Num. Firms)**	**Share of 2010 Construction Sector: Value Added**	**FTE Workers**
AL	State Website	✗	196	15.7%	17.4%
AR	State Website	✗	149	7.9%	12.8%
AZ	No	✗	*	*	*
CA	State Website	✗	1,041	8.3%	11.2%
CO	FOIA Request	✓	241	12.6%	14.7%
CT	FOIA Request	✗	126	9.4%	15.5%
FL	State Website	✓	344	30.7%	10.6%
GA	BidX Website	✗	137	4.3%	7.0%
IA	BidX Website	✗	256	15.4%	20.7%
ID	BidX Website	✗	112	17.2%	13.6%
IL	No	✗	*	*	*
IN	State Website	✓	213	10.6%	16.6%
KS	BidX Website	✓	130	13.7%	21.6%
KY	No	✗	*	*	*
LA	BidX Website	✗	167	11.5%	10.8%
MA	No	✗	*	*	*
MD	No	✗	*	*	*
ME	BidX Website	✗	141	13.7%	16.9%
MI	BidX Website	✗	391	9.5%	16.3%
MN	BidX Website	✗	262	13.5%	19.8%
MO	BidX Website	✗	179	14.9%	13.3%
MS	No	✗	*	*	*
MT	FOIA Request	✗	122	15.0%	23.6%
NC	BidX Website	✗	135	5.2%	9.8%
ND	FOIA Request	✗	*	*	*
NE	No	✗	*	*	*
NH	No	✗	*	*	*
NJ	No	✗	*	*	*
NM	BidX Website	✗	*	*	*
NV	No	✗	*	*	*
NY	No	✗	*	*	*
OH	BidX Website	✗	320	43.7%	17.5%
OK	No	✗	*	*	*
OR	No	✗	*	*	*
PA	No	✗	*	*	*
SC	No	✗	*	*	*
SD	No	✗	*	*	*
TN	BidX Website	✗	140	5.3%	11.5%
TX	FOIA Request	✓	551	4.9%	9.6%
UT	No	✗	*	*	*
VA	BidX Website	✗	241	14.2%	12.0%
VT	BidX Website	✗	*	*	*
WA	BidX Website	✗	200	7.5%	14.0%
WI	BidX Website	✗	194	12.1%	14.6%
WV	BidX and State Websites	✓	103	13.7%	19.0%
National			6,792	10.7%	9.9%

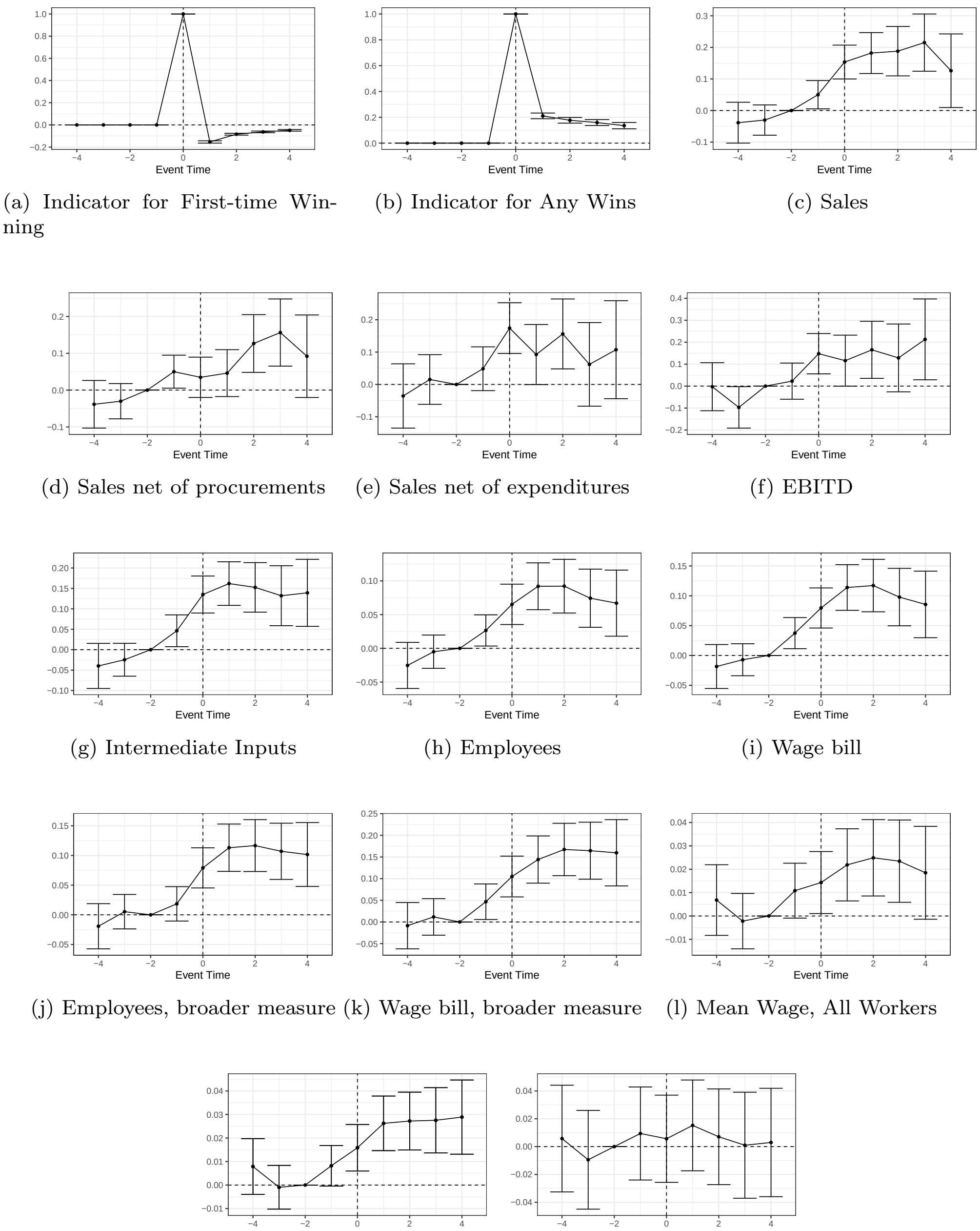
(b) Shares by State of the Matched Firms in 2010

	Value Per Firm ($ millions except workers)	Mean of the Log (2010)	Mean of the Log (All Years)	Share of the Construction Sector (%)
Sales	19.927	15.061	15.291	12.1%
Profits	9.159	14.075	14.075	9.6%
Intermediate Costs	14.661	14.719	14.958	12.4%
Wage bill	2.737	13.549	13.682	13.4%
Workers (individuals)	45.976	2.812	2.914	11.7%

(c) Characteristics of the Matched Firms in 2010

Table A.4: Characteristics of the Procurement Auctions Sample

Notes: These table characterizes the matched sample of firms that bid in procurement auctions. Table (a) provides information on the performance of various matching algorithms. Table (b) provides information on the representativeness of the matched sample in 2010, where states with more than zero but fewer than 100 matched firms are omitted (denoted by *). Table (c) displays sample statistics (in 2010 unless stated otherwise). The full matched sample includes 7,876 firms, comprising 0.9% of construction industry firms in 2010. For reference, workers per firm in the full sample is 38.7, median wage bill per worker is $47.1 thousand, and median profits per worker is $20.1 thousand. Intermediate costs are used in constructing the profits measure, and the variance of log intermediate costs is 3.0652 while the covariance between log intermediate costs and log sales is 2.559.



(a) Indicator for First-time Winning

(b) Indicator for Any Wins

(c) Sales

(d) Sales net of procurements

(e) Sales net of expenditures

(f) EBITD

(g) Intermediate Inputs

(h) Employees

(i) Wage bill

(j) Employees, broader measure

(k) Wage bill, broader measure

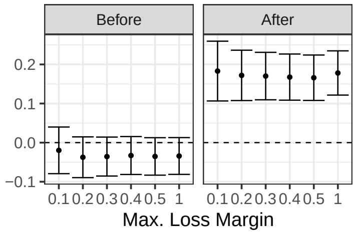
(l) Mean Wage, All Workers

(m) Mean Wage, Stayers

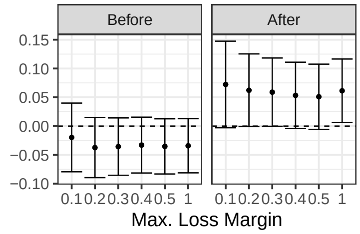
(n) Mean Previous Earnings, New Hires

Figure A.5: Pass-through of Observable Demand Shocks

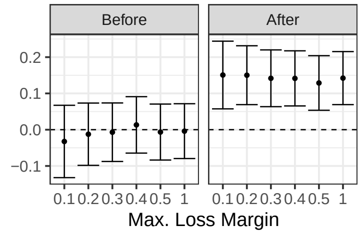
Notes: This figure presents average treatment effect on the treated estimates using the difference-in-differences specification defined in the text. All results include firm fixed effects. The control units are those firms that place a bid in a procurement auction in the same year that the reference treatment cohort wins. The omitted relative time is -2. 90% confidence intervals are displayed, clustering on firm.



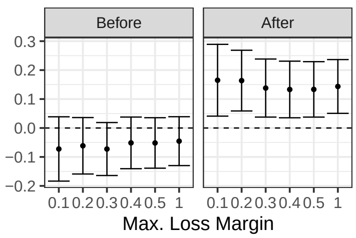
(a) Sales



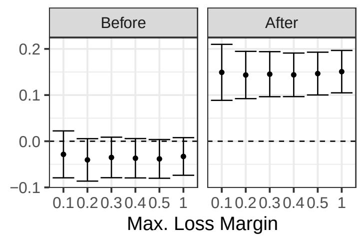
(b) Sales net of procurements



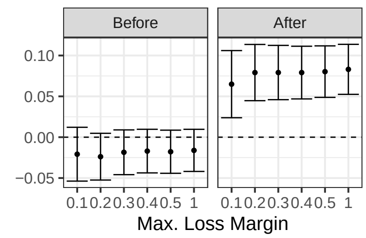
(c) Sales net of expenditures



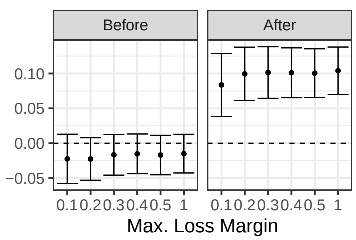
(d) EBITD



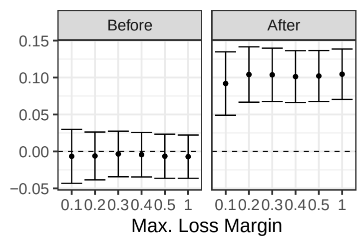
(e) Intermediate Inputs



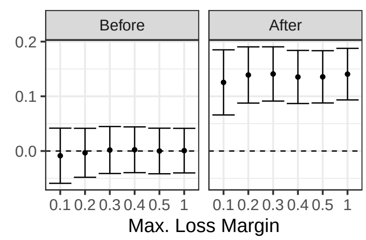
(f) Employees



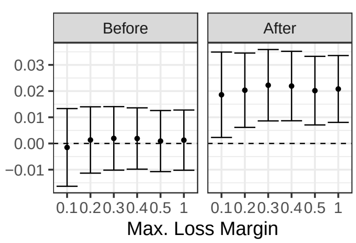
(g) Wage bill



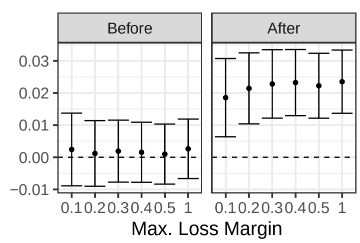
(h) Employees, broader measure



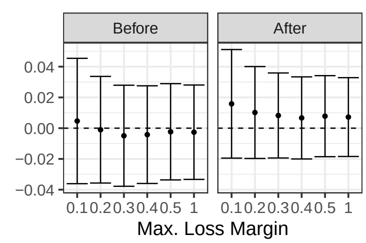
(i) Wage bill, broader measure



(j) Mean Wage, All Workers



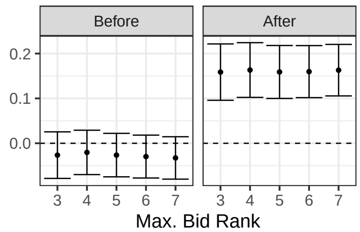
(k) Mean Wage, Stayers



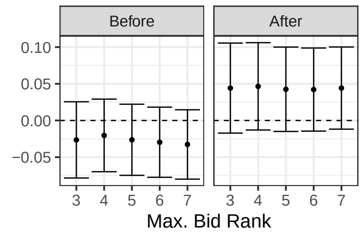
(l) Mean Previous Wage, New Hires

Figure A.6: Robustness of Observable Pass-through: Restricting the Control Group's Bid Loss Margin

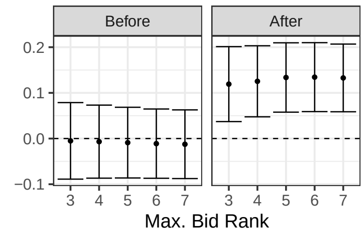
Notes: This figure presents average treatment effect on the treated estimates using the difference-in-differences specification defined in the text. All results include firm fixed effects. "Before" refers to relative times {-4,-3,-2} and "After" refers to relative times {0,1,2}. The Baseline sample restricts the control units to those that place a bid in a procurement auction in the same year that the reference treatment cohort wins. We then restrict the control group to firms whose bid loss margin was lower than the number displayed on the x-axis.



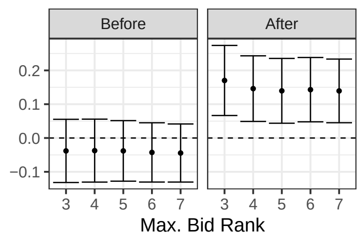
(a) Sales



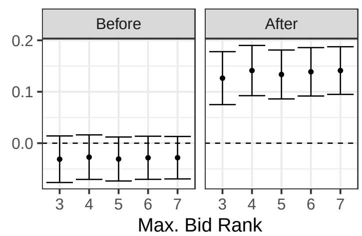
(b) Sales net of procurements



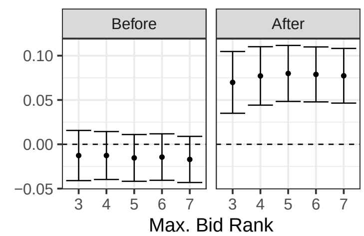
(c) Sales net of expenditures



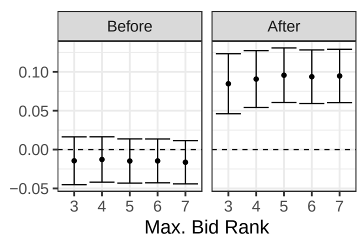
(d) EBITD



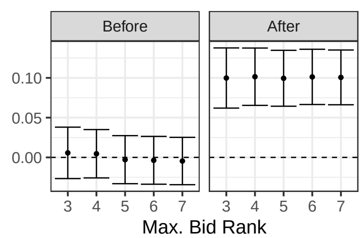
(e) Intermediate Inputs



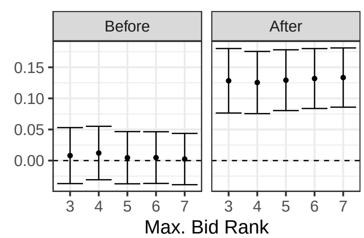
(f) Employees



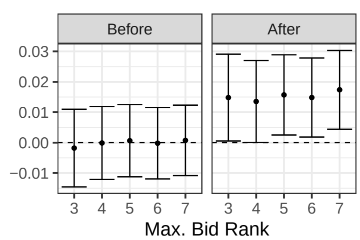
(g) Wage bill



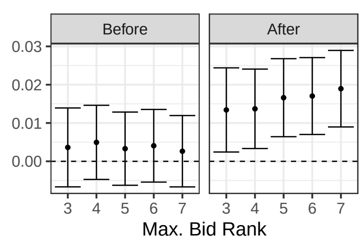
(h) Employees, broader measure



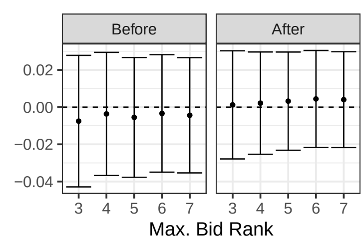
(i) Wage bill, broader measure



(j) Mean Wage, All Workers



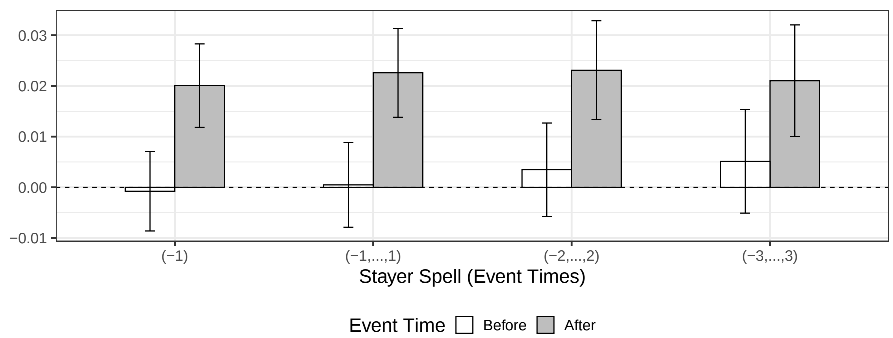
(k) Mean Wage, Stayers



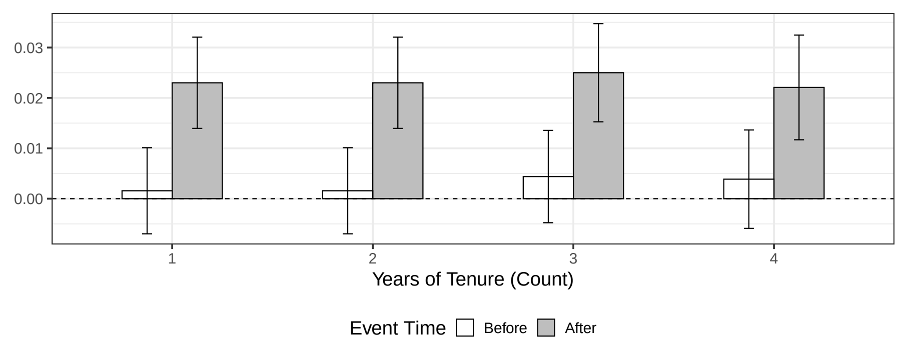
(l) Mean Previous Wage, New Hires

Figure A.7: Robustness of Observable Pass-through: Restricting the Control Group's Bid Ranks

Notes: This figure presents average treatment effect on the treated estimates using the difference-in-differences specification defined in the text. All results include firm fixed effects. "Before" refers to relative times {-4,-3,-2} and "After" refers to relative times {0,1,2}. The Baseline sample restricts the control units to those that place a bid in a procurement auction in the same year that the reference treatment cohort wins. We then restrict the control group to firms whose bid rank was less than or equal to the number displayed on the x-axis.



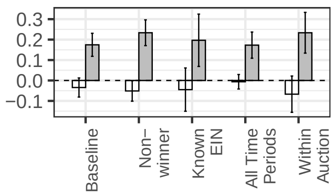
(a) Robustness by Stayer Spell



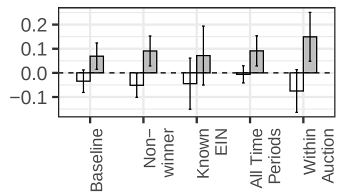
(b) Robustness by Tenure Length

Figure A.8: Robustness of Observable Pass-through: Stayer and Tenure Samples

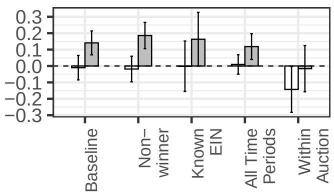
Notes: This figure presents average treatment effect on the treated estimates using the difference-in-differences specification defined in the text. All results include firm fixed effects. “Before” refers to relative times {-4,-3,-2} and “After” refers to relative times {0,1,2}. The Baseline sample restricts the control units to those that place a bid in a procurement auction in the same year that the reference treatment cohort wins. The omitted relative time is -2. 90% confidence intervals are displayed, clustering on firm. Subfigure (a) varies the window in which the worker must have been employed by the bidding firm, where (-2,...,2) is treated as the baseline definition of stayers. Subfigure (b) varies the window over which the worker must have been employed prior to the auction bid, e.g., tenure of -4 means that the worker was employed from relative time -4 until at least relative time 0.



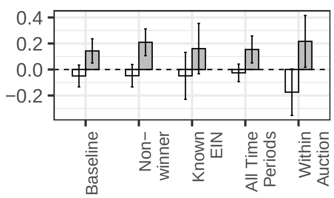
(a) Sales



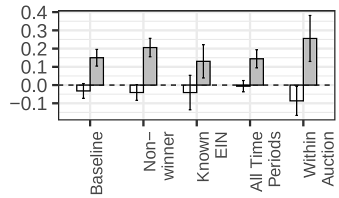
(b) Sales net of procurements



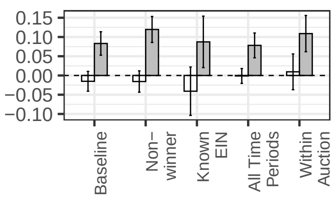
(c) Sales net of expenditures



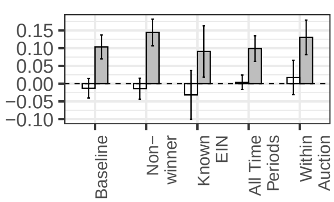
(d) EBITD



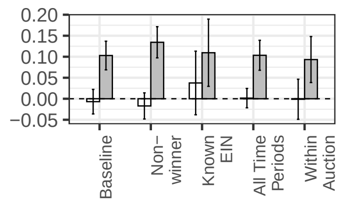
(e) Intermediate Costs



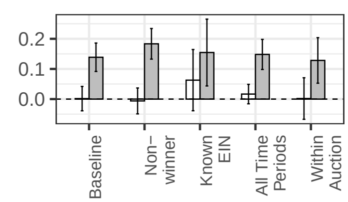
(f) Employees



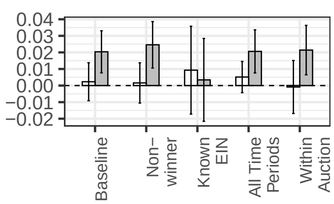
(g) Wage bill



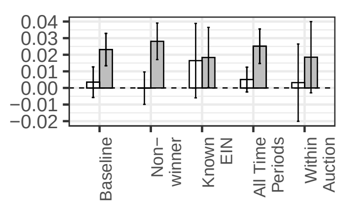
(h) Employees, broader measure



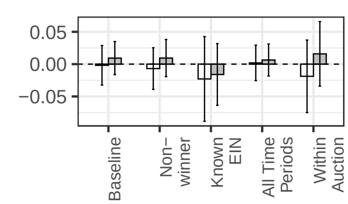
(i) Wage bill, broader measure



(j) Mean earnings, all employees



(k) Mean earnings, stayers



(l) Mean past earnings, new hires

Figure A.9: Robustness of Observable Pass-through: Various Alternative Specifications

Notes: This figure presents average treatment effect on the treated estimates using the difference-in-differences specification defined in the text. All results include firm fixed effects. "Before" refers to relative times {-4,-3,-2} and "After" refers to relative times {0,1,2}. In "Baseline", "Before" refers to relative times {-4,-3,-2} and "After" refers to relative time {0,1,2}. In "All Time Periods", "Before" refers to relative times {-4,-3,-2,-1} and "After" refers to relative time {0,1,2,3,4}.The omitted relative time is -2. 90% confidence intervals are displayed, clustering on firm.

C Appendix: Movers Analyses

C.1 Limited mobility bias

Even if the restrictions discussed in the text hold, it is challenging to draw inference about the inequality contribution from firm effects and worker sorting. A key challenge is the incidental parameter bias caused by the large number of firm-specific parameters that are solely identified from workers who move across firms. The analysis of Andrews et al. (2008) suggests this limited mobility bias can be substantial. With few movers per firm, the firm component is biased upwards while the sorting component is biased downwards, with the size of the bias depending inversely on the degree of worker mobility among firms.

To get a better sense of the scope for limited mobility bias in the U.S. data, we would ideally apply the AKM estimator to alternative samples of workers and firms that are comparable except for the number of movers per firm. Figure A.10 presents the results from such an analysis, suggesting that the variance of firm effects declines monotonically as the number of movers per firm increases. To construct this figure, we consider a subsample of firms with reasonably many movers; that is, at least 15 movers per firm over the period 2001-2008. Applying AKM to this subsample gives an estimate of the variance of firm effects of 6.7 percent. Next, we remove movers randomly within firms (keeping the connected set of firms approximately the same) before re-estimating the AKM model. The solid line displays the AKM estimates of the variance of firm effects after randomly removing movers. Consistent with limited mobility bias, the fewer the number of movers per firm, the larger the variance of firm effects. For approximately the same set of firms, the estimated variance of firm effects is several times as large (23 percent) if we only keep ten percent of the movers within each firm (on average, 7 movers per firm) as compared to what we obtained if we keep all the movers per firm (at a minimum 15 and, on average, 62 movers per firm). By way of comparison, there are around 18 movers per firm in the full estimation sample (which roughly corresponds to the number of movers per firm when randomly removing 40% of movers).

Until recently, the procedures for addressing limited mobility bias required strong and questionable assumptions about the covariance structure of the time-varying errors (see e.g. the discussion in Card et al., 2018). To address this shortcoming, BLM and Kline et al. (2018b) propose approaches to address limited mobility bias that rely on a different or weaker set of assumptions.[1] The first approach reduces the dimension of firm heterogeneity to a finite number of types. BLM show how this approach can be used to alleviate the biases arising from low mobility rates. The second approach uses a version of the Jackknife method. Kline et al. (2018b) show how this approach allows one to relax the homoskedasticity assumption in the bias correction procedure proposed by Andrews et al. (2008). Since it is computationally infeasible to apply Andrews et al. (2008) and Kline et al. (2018b) to very large data sets (as one needs to compute the trace of the inverse of the mobility matrix), our main analysis is based on the approach of BLM. As a robustness check, however, we use a subset of the U.S. states to assess

[1] Another possibility is to change the definition of a firm effect. See Borovickova and Shimer (2017) for such an approach.

the sensitivity of the results to the choice of procedure for addressing limited mobility bias.

In Figure A.10, the dotted line shows estimates of the variance of firm effects based on the procedure of BLM that addresses limited mobility bias. Firms are first classified into groups based on the empirical earnings distribution using the k-means clustering algorithm. The k-means classification groups together firms whose earnings distribution is most similar. Then, in a second step, the worker effects and firm effects are estimated. While the specification of BLM in Figure A.10 assumes there exists 10 firm types, Appendix Figure A.11 shows the BLM estimates do not materially change if we instead allow for 20, 30, 40 or 50 firm types. Consistent with limited mobility bias, the BLM estimates are noticeably smaller than the standard AKM estimates in the samples with few movers. As expected, the AKM estimates become more similar to the BLM estimates when there is a large number of movers per firm, and thus, limited mobility bias should be small.

C.2 Extensions to the AKM Model

The assumptions that $\phi_{ij} = x_i + \psi_j$ and $\gamma = \Upsilon = 0$ implies strong restrictions on the wage structure. The absence of interactions between worker and firm effects rules out strong complementaries in production, as in Shimer and Smith (2000) and Eeckhout and Kircher (2011). The assumption of no pass through of firm and market shocks is at odds with our data and a large body of evidence from many other developed countries. Thus, investigating these assumptions seems important to draw credible conclusions about the functioning of the U.S. labor market.

Non-additivity and complementarities

The assumption that $\phi_{ij} = x_i + \psi_j$ implies that all workers who move from firm j to j' will experience an earnings change of $\psi_{j'}$ - ψ_j, no matter their quality x_i. An informal way to assess this log additive structure is to perform an event study of the earnings changes experienced by workers moving between different types of firms. Card et al. (2013b) and Card et al. (2018) use matched employer-employee data from Germany and Portugal to perform such event-study analyses of the earnings changes experienced by workers moving between different types of firms. In Appendix Figure A.12, we perform the same exercise, but this time for our U.S. data. This analysis uses the movers sample. As in Card et al. (2013b) and Card et al. (2018), we define firm groups based on the average pay of coworkers.

The results from the event study mirror those reported in Card et al. (2013b) and Card et al. (2018). Workers who move to firms with more highly-paid coworkers experience earnings raises, while those who move in the opposite direction experience earnings decreases of similar magnitude. Additionally, the gains and losses for movers in opposite directions between any two groups of firms are relatively symmetric. By comparison, earnings do not change materially when workers move between firms with similarly paid coworkers. Another relevant finding from the event study is that the earnings profiles of the various groups are all relatively stable in the years before and after a job move. This lends support to Assumption 2, as it suggests that worker mobility does not seem to depend strongly on the trends in earnings beforehand or afterwards. Lastly, it is interesting to observe that the gains and losses for movers seem to be permanent. In contrast, in a large class of search models with job ladders, moves to firms that currently pay less is rationalized by arguing that these firms will pay more in the future.

Although the event study results are consistent with the log additive functional form, we cannot rule out interaction effects between worker and firm effects. Indeed, Bonhomme et al. (2019) point out that even if the functional form is non-additive, the gains and losses may look symmetric if workers making upward moves are of the same quality as those making downward moves. More generally, the degree of asymmetry one observes in the event study depends both on the magnitudes of any interaction effects and on the extent to which workers making upward moves differ in quality from those making downward moves. Thus, the event study analysis needs to be interpreted with caution.

To obtain an actual estimate of the importance of interactions between worker and firm effects, we follow BLM in using the following model of earnings:

$$w_{it} = \underbrace{\theta_{j(i,t)} \cdot x_i}_{\text{interaction}} + \psi_{j(i,t)} + \epsilon_{it} \tag{10}$$

which reduces to AKM when θ_j is the same for all firms. Under Assumptions 1 and 2 , we obtain:

$$\begin{aligned}
\mathbb{E}[w_{it+1}|j_2 \to j_1] - \mathbb{E}[w_{it}|j_1 \to j_2] &= \theta_{j_1} \left(\mathbb{E}\left[x_i|j_2 \to j_1\right] - \mathbb{E}\left[x_i|j_1 \to j_2\right]\right) \\
\mathbb{E}[w_{it+1}|j_1 \to j_2] - \mathbb{E}[w_{it}|j_2 \to j_1] &= -\theta_{j_2} \left(\mathbb{E}\left[x_i|j_2 \to j_1\right] - \mathbb{E}\left[x_i|j_1 \to j_2\right]\right)
\end{aligned}$$

where $j_1 \to j_2$ ($j_2 \to j_1$) is an indicator for a worker moving from firm 1 to 2 (firm 2 to 1). As long as the workers moving from 1 to 2 are not exactly the same as those moving from 1 to 2, the right hand side of these equalities are non-zero and we can recover θ_{j1}/θ_{j2} from the moment condition:

$$\frac{\mathbb{E}[w_{it+1}|j_2 \to j_1] - \mathbb{E}[w_{it}|j_1 \to j_2]}{\mathbb{E}[w_{it}|j_2 \to j_1] - \mathbb{E}[w_{it+1}|j_1 \to j_2]} = \frac{\theta_{j1}}{\theta_{j2}} \tag{11}$$

Thus, provided that the composition of movers differs across firms, it is possible to identify θ_j (up to scale) for every firm. To take (10) to the data, however, it is useful to reduce the number of parameters to estimate. As above, we follow BLM in classifying firms to ten types according to the empirical earnings distribution within firms. Then we restrict θ_j to be the same for all firms of a given type.

Figure A.13 displays the estimated nonlinearities. We plot the means of log earnings for each firm type and at 10 deciles of worker heterogeneity. On the x-axis, firm types are ordered in ascending order, where "lower" and "higher" types refer to low and high mean log earnings. The results show clear evidence of worker heterogeneity: For the same type of firm, better workers earn significantly more. For a given worker, there is also some variation in log earnings between firm types, although to a lesser extent. As shown in equation (11), the parameters governing nonlinearities are identified from comparing the gains from moving from a low to a high type of firm for workers of different quality. As evident from Figure A.13, the gains from such a move are considerably larger for better workers. For example, moving from the lowest to the highest type of firm increases earnings by 22, 47 and 78 percentage points for individuals at the 20, 50 and 80 percentile in the worker quality distribution.

The evidence of nonlinearities raises several questions. To what extent do interaction effects bias the estimates from the log additive model? Are nonlinearities empirically important as a source of earnings inequality? In Table A.7, we investigate these questions by extending the AKM decomposition to incorporate the contribution from interactions between worker and firm effects. Re-arranging equation (10), we get

$$w_{it} = \underbrace{\bar{\theta}(x_i - \bar{x})}_{\tilde{x}_i} + \underbrace{\left(\psi_{j(i,t)} + \theta_{j(i,t)}\bar{x}\right)}_{\tilde{\psi}_{j(i,t)}} + \underbrace{(\theta_{j(i,t)} - \bar{\theta})(x_i - \bar{x})}_{\varrho_{ij(i,t)}} + \epsilon_{it} \tag{12}$$

where $\bar{\theta} \equiv \mathbb{E}\left[\theta_{j(i,t)}\right]$ and $\bar{x} \equiv \mathbb{E}\left[x_i\right]$. This equation decomposes the earnings of worker i in period t into three distinct components: $\tilde{x}_i$ gives the direct effect of the quality of worker i (evaluated at the average firm), $\tilde{\psi}_{j(i,t)}$ represents the direct effect of firm j (evaluated at the average worker), and $\varrho_{ij(i,t)}$ captures the interaction effect between firm j and worker i quality.

Using equation (12), we obtain a new variance decomposition of log earnings:

$$\begin{aligned} Var(w_{it}) = & Var\left[\tilde{x}_i\right] + Var\left[\tilde{\psi}_{j(i,t)}\right] + 2Cov\left[\tilde{x}_i, \tilde{\psi}_{j(i,t)}\right] \\ & + Var\left[\varrho_{ij(i,t)}\right] + 2Cov\left[\tilde{x}_i + \tilde{\psi}_{j(i,t)}, \varrho_{ij(i,t)}\right] \end{aligned} \tag{13}$$

The first three components are informative about the inequality contribution from worker effects, firm effects and worker sorting, net of interaction effects. The last two components are informative about the inequality contribution from interaction effects, as measured by the dispersion of $\varrho_{ij(i,t)}$ across firms and the extent to $\varrho_{ij(i,t)}$ is larger in firms with high wages. If $\theta_j = \bar{\theta}$ for every firm j, then these two components would be zero, and the decomposition in (13) reduces to the standard AKM decomposition.

The results from the decomposition in (13) are presented in column (2) of Table A.7. Our estimates suggest the dispersion of interaction effects across firms explains three percent of the earnings inequality. However, the total contribution to earnings inequality from nonlinearities is muted by the interaction effects being larger in firms with higher paid workers. We also find that omitting interaction effects causes a downward bias in the firm effects and an upward bias in the worker effects.

Pass through of shocks and time-varying types

The assumption that $\gamma = \Upsilon = 0$ restricts firm effects to be constant over time. However, the significant pass-through rates imply that firm effects actually evolve over time as employers experience changes in the value added at the firm or market level. To capture this, we now let γ differ from Υ and propose an adjustment to the AKM model which allows us to isolate the time-invariant component of the firm effects.

Our approach proceeds in two steps. First, we construct an adjusted earnings measure by removing the time-varying firm and market specific component of earnings. To do so, we use the firm and market level value added multiplied by the estimated passthrough coefficients at the firm and market level (see Table 2). Second, we recover the time-invariant firm and worker effects by applying the methods of AKM or BLM to the adjusted measure of earnings. Consider the following adjusted two-way specification for earnings of workers across firms:

$$\mathbb{E}[w_{it} - \gamma(y_{j(i,t),t} - y_{j(i,t),1}) - (\Upsilon - \gamma)\left(\bar{y}_{r(i,t),t} - \bar{y}_{r(i,t),1}\right)|j(i,1),...,j(i,T)] = x_i + \psi_j.$$

The left-hand side removes the earnings dynamics due to passthrough of firm-specific shocks, $\gamma(y_{j(i,t),t} - \bar{y}_{r(i,t),t})$, and market shocks, $\Upsilon\bar{y}_{r(i,t),t}$. What remains is the worker effect x_i and the time-invariant firm effect ψ_j, which can be estimated by applying AKM or BLM to the adjusted earnings measure.

In column (3) of Table A.7, we extend the BLM decomposition of the variance of log earnings in (2) to incorporate the contribution from time-invariant and time-varying firm effects. We find that time-varying firm effects explain little if any of the variation in log earnings, and that the importance of firm effects and worker sorting do not change materially if we take the pass through of firm shocks into account. Comparing the results in column (4) to those presented in column (2) shows that time variation also has little to no explanatory power when accounting for nonlinearities.

C.3 Additional robustness checks

In our main analysis, we follow the literature in looking at individuals aged 25-60 for whom earnings exceed the full-time equivalence. This raises the question of how sensitive the results are to changing these sample selection criteria. In Appendix Figure A.15, we re-estimate the AKM model with alternative employment definitions, reporting the variance of log earnings and the firm effects. This figure also examines how the results change if we include workers aged 20-25. As expected, the variance of log earnings and the estimated firm effects increase if we include individuals earning less than the full-time equivalence. By comparison, the inclusion of younger workers do not materially change the estimates.

We repeat many of the movers analyses for the "Broad Sample". This sample is similar to the Baseline Sample considered elsewhere, but with five differences. First, firms are not required to have positive value added. Second, firms are not required to have at least two movers. Third, locations are taken from worker rather than firm forms. Fourth, we consider shorter time intervals of 2, 3, or 6 years rather than 8 years. Fifth, because the sample is smaller, we can feasibly compare many alternate estimation strategies.

Characteristics of the Broad Sample are displayed in Table A.8(a). We see that, when using a smaller numbers of years, there are fewer movers relative to the number of stayers, on average as well as across the distribution of movers. This results in a much smaller share of firms belonging to the connected set. Table A.8(a) also characterizes the leave-one-out set of Kline et al. (2018b), which requires that firms are connected by at least one mover even after dropping a mover from the sample and is required to use the bias correction method of Kline et al. (2018b).

The main national results for the Broad Sample are displayed in Appendix Tables A.8(b) for the connected set and A.8(c) for the leave-one-out set. In the connected set, the AKM estimator (FE) estimates a variance of firm effects of 12% to 16%, which are greater than the estimate of 9% in the Baseline Sample, and a sorting share of -12% to 1%, which are less than 5% in the Baseline Sample, which is consistent with greater bias if there are fewer movers per firm. Imposing the leave-one-out set, the FE estimates become more similar to the Baseline Sample, as the number of movers per firm rises. Applying three types of bias correction procedures by Bonhomme et al. (2019) (CRE), Andrews et al. (2008) (FE-HO), and Kline et al. (2018b) (FE-HE), we find that the variance of firm effects ranges from 4% to 6%, which is similar to the 3% bias-corrected estimate in the Baseline Sample.

Using the Broad Sample, we repeat the various exercises presented above. Appendix Figure A.12 compares the event study of earnings changes for workers who move across firms in the Baseline Sample (subfigure a) and the Broad Sample (subfigure b), finding strong similarities. Appendix Figure A.11 compares BLM and CRE estimates as the number of clusters increases from 10 to 50 for the Baseline Sample (subfigure a) and the Broad Sample (subfigure b), finding that the number of clusters does not affect the estimates. Using the Broad Sample to investigate the full-time earnings threshold, we find in Appendix Figure A.17(a-b) a similar pattern as we found in Appendix Figure A.15 for the Baseline Sample. In Appendix Figure A.17(c-d), we examine the sensitivity of the estimates to the minimum number of workers per firm, finding that much of the bias in the FE estimator dissipates if only considering larger firms. Using the Broad Sample to investigate limited mobility bias by share of movers kept, we find in Appendix Figure A.17(e-h) a similar pattern as we found in Appendix Figure A.10 for the Baseline Sample.

We consider several additional robustness checks that make use of the Broad Sample. Appendix Figure A.17(i-j) shows that the qualitative results for the Baseline Sample and the Broad Sample are present even when considering very short 2-year panels. Appendix Figure A.17(k-l) demonstrates that the bias in the AKM estimator would become even stronger if we used a strict definition of movers in which we require workers to be employed for three consecutive years at each firm. Appendix Figure A.17(m-n) considers the 20 smallest states, showing that the main qualitative results hold at the state level, while Appendix Figure A.17(o-p) shows that these results hold at the state level even when using the exact solution to the estimator rather than the approximation method required for feasible estimation on large samples.

C.4 Tables and Figures

Sample:	Full Sample	≥ 2 Movers	Connected Set
Workers in 2001-2008:			
Worker-Years (Millions)	245.0	227.8	227.4
	(100.0%)	(93.0%)	(92.8%)
Unique Workers (Millions)	66.2	61.8	61.7
	(100.0%)	(93.3%)	(93.2%)
Workers in 2008-2015:			
Worker-Years (Millions)	232.9	212.4	211.9
	(100.0%)	(91.2%)	(91.0%)
Unique Workers (Millions)	64.0	58.8	58.6
	(100.0%)	(91.9%)	(91.7%)

Table A.5: Floor on Number of Movers and the Connected Set

Notes: This table demonstrates the fraction of workers lost from the sample in the AKM and BLM analysis when imposing that a firm must have at least two movers and must belong to the connected set of firms.

Years:	**2001-2008**	**2008-2015**	**Pooled**
Panel A.		**Levels**	
Total SD	0.67	0.68	0.67
Worker Effects SD	0.57	0.58	0.57
Firm Effects SD	0.20	0.20	0.20
Covariates SD	0.11	0.12	0.11
Correlation: x_i and $\psi_{j(i)}$	0.09	0.11	0.10
Correlation: x_i and $X_i'b$	0.00	0.00	0.00
Correlation: $X_i'b$ and $\psi_{j(i)}$	0.02	0.03	0.03
Panel B.		**Percentages**	
$Var(x_i + X_i'b)$	75.4%	75.5%	75.4%
$Var(x_i)$	72.9%	72.4%	72.6%
$Var(X_i'b)$	2.5%	3.1%	2.8%
$2Cov(x_i, X_i'b)$	0.0%	0.0%	0.0%
$Var(\psi_{j(i)})$	8.8%	9.0%	8.9%
$2Cov(x_i + X_i'b, \psi_{j(i)})$	4.9%	5.7%	5.3%
$2Cov(x_i, \psi_{j(i)})$	4.6%	5.4%	5.0%
$2Cov(X_i'b, \psi_{j(i)})$	0.2%	0.3%	0.3%
Residual	11.0%	9.9%	10.4%

Table A.6: Detailed AKM Decomposition

Notes: This table decomposes the variance of log earnings into components of worker effect variance, firm effect variance, the variance of time-varying observables, and the covariances among these components. Results are presented for the AKM estimator for the 2001-2008 and 2008-2015 samples, as well as their average (Pooled).

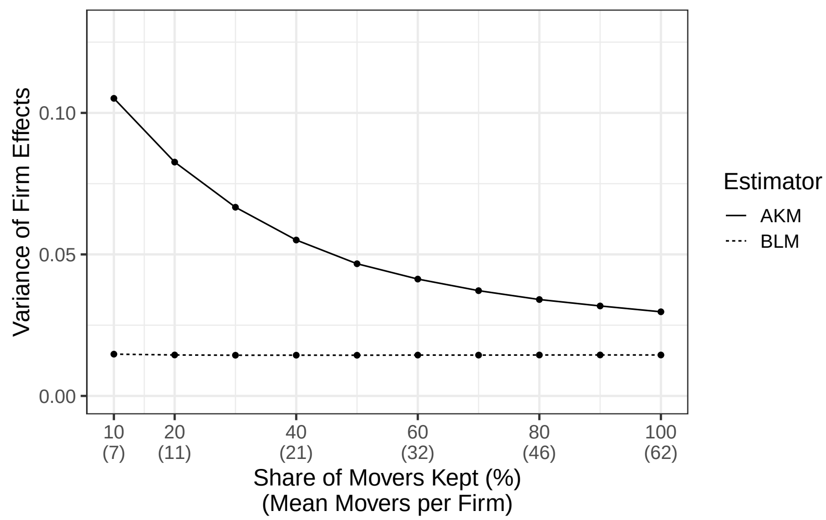
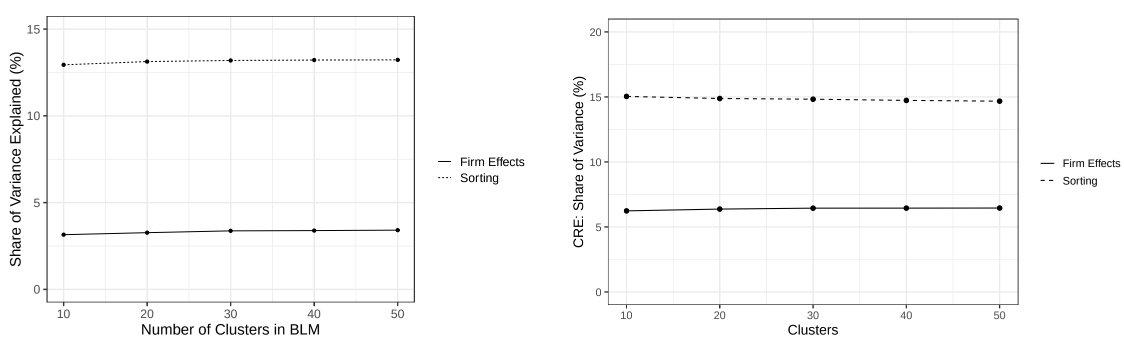


Figure A.10: Empirical Characterization of Limited Mobility Bias

Notes: In this figure, we consider the subset of firms with at least 15 movers. We randomly remove movers within each firm and re-estimate the variance of firm effects using the AKM and BLM estimators. For each estimator, we repeat this procedure several times, and then take averages of the variance estimates across these repetitions. The procedure allows us to keep the connected set of firms approximately the same and examine the bias that results from having fewer movers available in estimation.

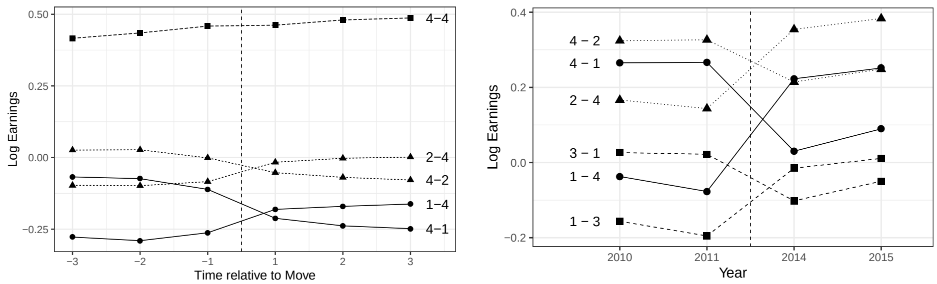


(a) Baseline Sample

(b) Broad Sample

Figure A.11: BLM Decomposition by Number of Clusters

Notes: In this figure, we estimate the BLM decomposition for different numbers of firm clusters.



(a) Baseline Sample

(b) Broad Sample

Figure A.12: Assessing Identifying Assumptions: Evidence on Symmetric Changes around the Move

Notes: In this figure, we classify firms into four equally sized groups based on the mean earnings of stayers in the firm (with 1 and 4 being the group with the lowest and highest mean earnings, respectively). We then compute mean log earnings for the workers that move between these groups of firms in the years before and after the move. Note that the employer differs between event times -1 and 1, but we do not know exactly when the change in employer occurred. Thus, to avoid concerns over workers exiting and entering employment during these years, one might prefer to compare earnings in event years -2 and 2.

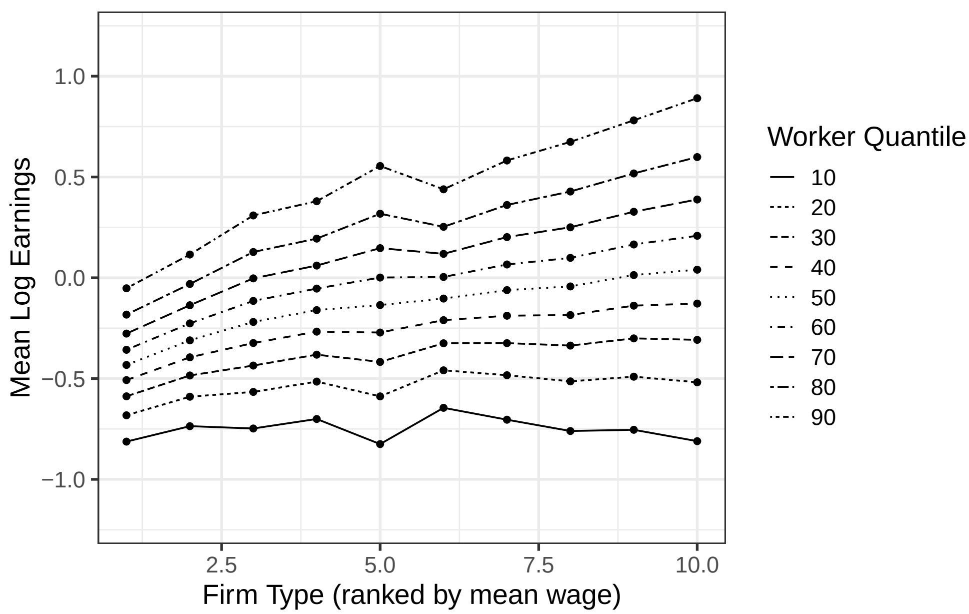


Figure A.13: Assessing Identifying Assumptions: Evidence on Firm-Worker Interactions

Notes: In this figure, we present estimates of interactions between firm and worker effects using the BLM estimator. We plot the means of log earnings for each firm type and deciles of worker heterogeneity. On the x-axis, firm types are ordered in ascending order, where “lower” and “higher” types refer to low and high mean log earnings.

		Model Specification			
		(1)	(2)	(3)	(4)
Share explained by:					
i) Worker Quality	$Var(x_i)$	72.4%	70.4%	73.5%	71.6%
ii) Firm Effects	$Var(\psi_{j(i)})$	3.2%	4.3%	3.0%	4.3%
iii) Sorting	$2Cov(x_i, \psi_{j(i)})$	12.9%	13.1%	12.8%	13.1%
iv) Interactions	$Var(\varrho_{ij})$		3.0%		3.3%
	$+2Cov(x_i + \psi_{j(i)}, \varrho_{ij})$		-1.8%		-2.5%
v) Time-varying Effects	$Var(\psi_{j(i),t} - \psi_{j(i)})$			0.3%	0.3%
	$+2Cov(x_i, \psi_{j(i),t} - \psi_{j(i)})$				
Sorting Correlation:	$Cor(x_i, \psi_{j(i)})$	0.43	0.38	0.43	0.37
Variance Explained:	R^2	0.89	0.89	0.90	0.90
Specification:					
Firm-Worker Interactions		✗	✓	✗	✓
Time-varying Firm Effects		✗	✗	✓	✓

Table A.7: Comparison of BLM Specifications

Notes: This table presents the decomposition of log earnings variation into firm and worker effects using the BLM estimator for four specifications: baseline, allowing for worker effects to interact with firm effects ("Firm-Worker Interactions"), allowing for a time-varying component in the firm effects due to the pass through of value added shocks ("Time-varying Firm Effects"), and allowing for both interactions between firm and worker effects and time-varying firm effects. The analysis uses both workers who move between firms and stayers.

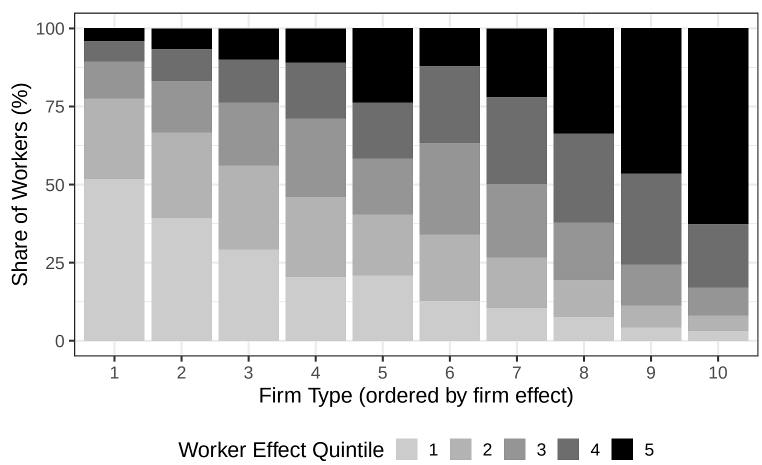


Figure A.14: Sorting of Firms and Workers

Notes: In this figure, we divide workers into 5 quintile bins and compute the share of workers in each quintile bin by firm class. We plot the firm classes in increasing order of firm effects.

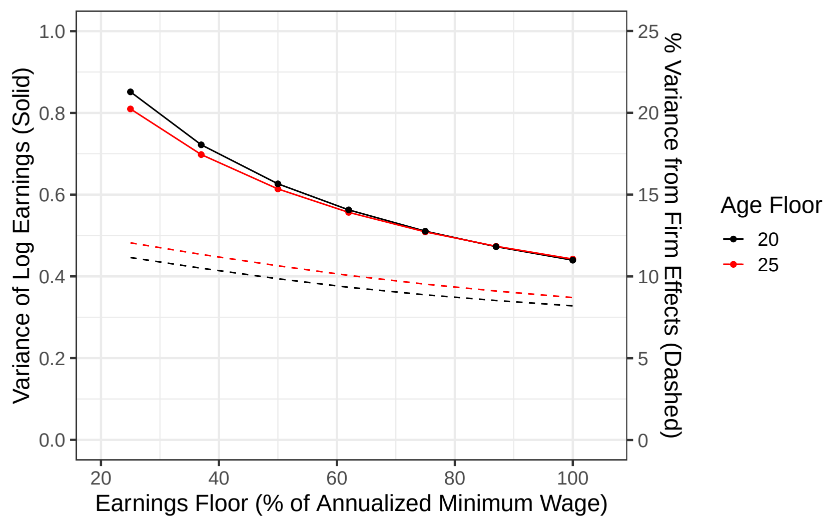


Figure A.15: Comparison of Log Earnings Variance by Earnings Floor

Notes: In this figure, we re-construct the full sample used to estimate the variance of log earnings (left y-axis) and the variance of AKM firm effects (right y-axis) when imposing different earnings floors and different age floors.

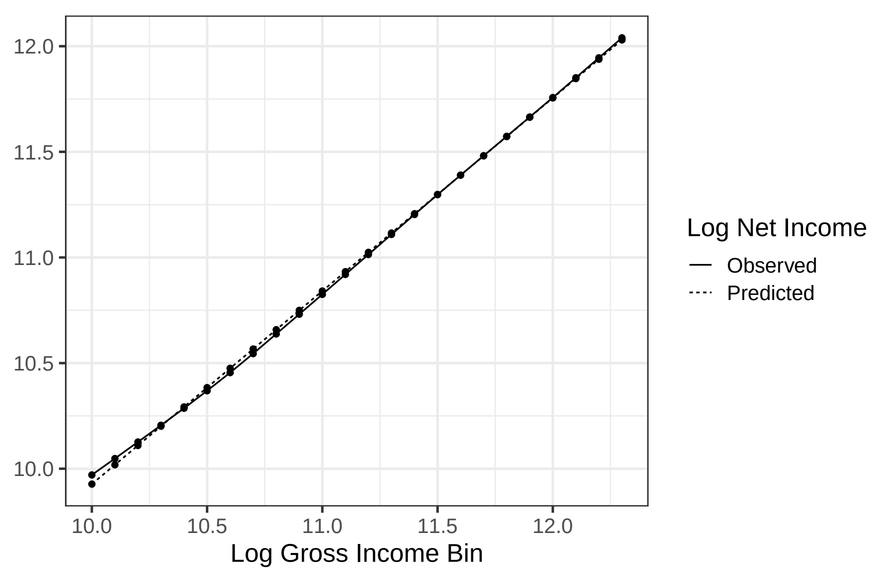


Figure A.16: Fit of the Tax Function

Notes: In this figure, we display the log net income predicted by the tax function compared to the log net income observed in the data.

Set:									
Baseline Years	2001-2006			2010-2012			2010-2015		
Full Set	✓	×	×	✓	×	×	✓	×	×
Connected Set	×	✓	×	×	✓	×	×	✓	×
Leave-one-out Set	×	×	✓	×	×	✓	×	×	✓
Sample Counts (1,000):									
Unique Firms	6,717	2,954	2,009	8,870	1,241	670	7,565	2,568	1,689
(Share of Full Set)	(100%)	(44%)	(30%)	(100%)	(14%)	(8%)	(100%)	(34%)	(22%)
Unique Workers	63,146	59,748	57,027	44,182	36,826	33,031	59,621	55,464	52,484
(Share of Full Set)	(100%)	(95%)	(90%)	(100%)	(83%)	(75%)	(100%)	(93%)	(88%)
Distribution of Moves:									
Moves per Firm	2.9	6.6	9.2	0.5	3.4	5.4	2.0	5.8	8.3
Worker-weighted quantiles:									
10th Quantile	4.0	4.0	6.0	2.0	2.0	3.0	3.0	4.0	5.0
50th Quantile	72.0	73.0	82.0	23.0	25.0	33.0	56.0	58.0	67.0
90th Quantile	6,226.1	6,277.1	6,560.6	1,604.3	1,649.5	1,822.5	4,214.2	4,304.3	4,675.8
Log Earnings Distrib.:									
Variance	0.397	0.395	0.395	0.432	0.436	0.440	0.413	0.414	0.416
Between-firm Share	34%	34%	33%	39%	38%	38%	40%	40%	39%

(a) Sample Characteristics

Panel A.	**Share of Total Variation**								
Years:	**2001-2006**			**2010-2015**			**2010-2015**		
	FE	**FE-HO**	**CRE**	**FE**	**FE-HO**	**CRE**	**FE**	**FE-HO**	**CRE**
Firm Effects	12.8%	6.5%	6.4%	16.3%	4.1%	5.2%	12.2%	5.5%	6.2%
Sorting	-0.7%	10.6%	12.1%	-12.0%	11.7%	12.5%	1.1%	13.5%	15.0%
Posterior Firm Effects			7.1%						6.7%
Panel B.	**Share of Between Firm Variation**								
Years:	**2001-2006**			**2010-2015**			**2010-2015**		
	FE	**FE-HO**	**CRE**	**FE**	**FE-HO**	**CRE**	**FE**	**FE-HO**	**CRE**
Firm Effects	37.3%	19.1%	18.7%	42.8%	10.7%	13.8%	30.9%	13.9%	15.8%
Sorting	-1.9%	31.1%	35.2%	-31.3%	30.7%	32.7%	2.7%	34.1%	38.0%
Segregation	64.6%	49.7%	46.1%	88.5%	58.6%	53.5%	66.3%	51.9%	46.3%

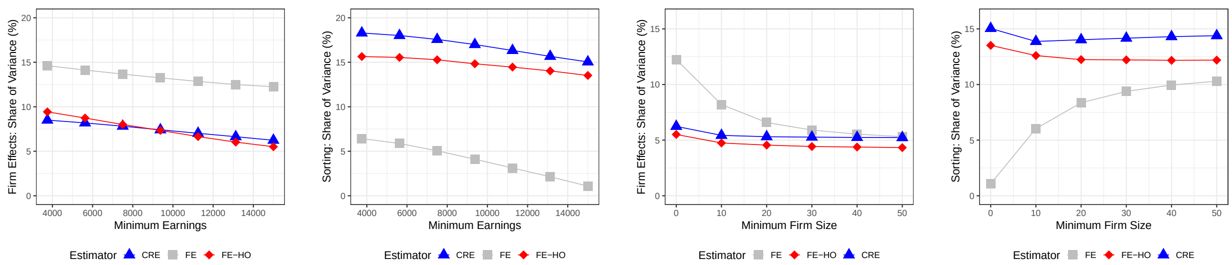
(b) Connected Set

Panel A.	**Share of Total Variation**											
Years:	**2001-2006**				**2010-2012**				**2010-2015**			
	FE	**FE-HO**	**FE-HE**	**CRE**	**FE**	**FE-HO**	**FE-HE**	**CRE**	**FE**	**FE-HO**	**FE-HE**	**CRE**
Firm Effects	10.2%	6.4%	6.7%	6.2%	10.4%	4.3%	4.5%	5.0%	9.5%	5.5%	5.8%	5.9%
Sorting	3.7%	10.4%	9.9%	11.7%	-0.8%	11.0%	10.6%	12.1%	5.9%	13.0%	12.5%	14.6%
Posterior Firm Effects				6.8%				5.2%				6.4%
Panel B.	**Share of Between Firm Variation**											
Years:	**2001-2006**				**2010-2012**				**2010-2015**			
	FE	**FE-HO**	**FE-HE**	**CRE**	**FE**	**FE-HO**	**FE-HE**	**CRE**	**FE**	**FE-HO**	**FE-HE**	**CRE**
Firm Effects	30.5%	19.3%	20.0%	18.6%	27.7%	11.3%	11.9%	13.2%	24.3%	14.2%	14.9%	15.3%
Sorting	11.2%	31.2%	29.8%	35.0%	-2.1%	29.3%	28.2%	32.2%	15.1%	33.5%	32.2%	37.6%
Segregation	58.3%	49.5%	50.3%	46.4%	74.4%	59.4%	59.9%	54.6%	60.6%	52.3%	52.9%	47.1%

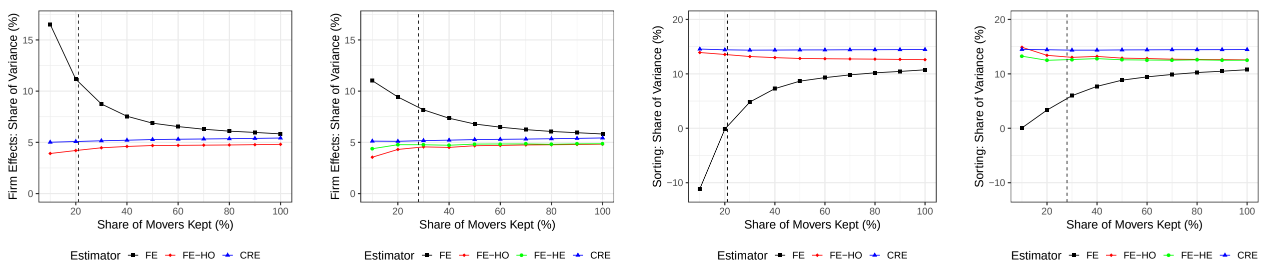
(c) Leave-one-out Set

Table A.8: Broad Sample - Total and Between Decompositions for Various Estimators

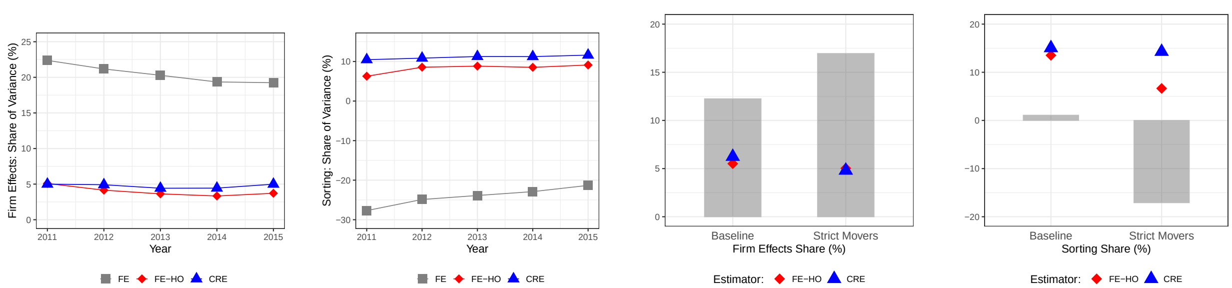
Notes: This table presents the decomposition of log earnings variation within and between firms using various estimators for three time intervals in the Broad Sample. The analysis uses both workers who move between firms and stayers.



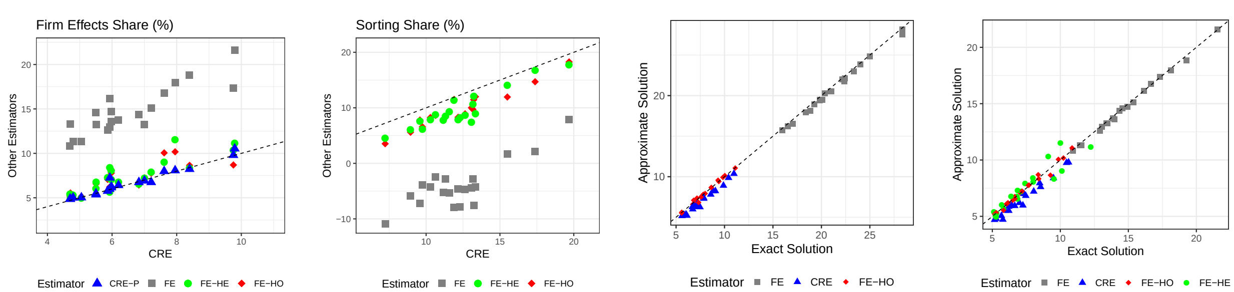
(a) Earnings Threshold: Firm Effects (b) Earnings Threshold: Sorting (c) Firm Size Threshold: Firm Effects (d) Firm Size Threshold: Sorting



(e) Limited Mobility Bias: Firm Effects (connected) (f) Limited Mobility Bias: Firm Effects (leave-one-out) (g) Limited Mobility Bias: Sorting (connected) (h) Limited Mobility Bias: Sorting (leave-one-out)



(i) Short Panels: Firm Effects (j) Short Panels: Firm Effects (k) Strict Movers: Firm Effects (l) Strict Movers: Sorting



(m) States: Firm Effects (leave-one-out) (n) States: Sorting (leave-one-out) (o) States: Approximation Methods (connected) (p) States: Approximation Methods (leave-one-out)

Figure A.17: Broad Sample - Various Robustness Checks

Notes: In this figure, we repeat several exercises shown previously, but now applied to the 2010-15 Broad Sample instead of the Baseline Sample (unless otherwise noted). See the text for details on the robustness exercises.

D Appendix: Analyses based on Income Shocks from Lottery Winnings

D.1 Identifying lottery-induced income effects

We begin by laying out the individual comparisons that allow us to estimate the effect of lottery income shocks by comparing lottery winners with earlier win years to those with later win years. Throughout, when we refer to individuals who win the lottery in the same calendar year, we will call them a cohort. To start, let Y_{it} be an outcome (such as earnings) observed for individual i in year t. Let E_i denote the year when individual i receives a lottery income shock. As a running example, consider the simple case with two groups – a group of winners winning in the year 2003 (the $E_i = 2003$ cohort) and all later winners (the $E_i > 2003$ cohorts). Suppose that for individuals in both groups, we have outcome data on year 2002 and 2003. In principle, we could calculate the average difference over time for the $E_i = 2003$ cohort and deem the average year-on-year change (e.g., $\mathbb{E}\left[Y_{i2003} - Y_{i2002} \middle| E_i = 2003\right]$) as the on-impact effect of lottery income shocks on the outcome. However, we may worry that events coinciding with the lottery or pre-existing trends in the outcome would bias our estimate of the effect of lottery income on the outcome with such a single difference. The existence of the later-winning group helps resolve these issues to the extent that it experiences similar evolution in the outcome over time (i.e., a parallel trend). Such a cohort is useful because its outcome is also observed in both 2002 and 2003, but it only receives a lottery shock in 2004 or later, meaning that any year-on-year change in the outcome for the $E_i > 2003$ group in 2002 and 2003 will be due to the pre-existing factors but not the (future) income shock[2]. Together, these suggest a difference-in-differences strategy to recover the effect of a lottery shock for the $E_i = 2003$ cohort in 2003:

$$\mathbb{E}\left[Y_{i2003} - Y_{i2002} \middle| E_i = 2003\right] - \mathbb{E}\left[Y_{i2003} - Y_{i2002} \middle| E_i > 2003\right] \tag{14}$$

With the availability of data on subsequent calendar years, we could recover a set of dynamic effects for the $E_i = 2003$ in all calendar years for which there remains a not-yet-treated cohort of individuals. Taking this logic further, for each E_i with any later-treated cohorts, we could estimate a set of cohort-specific dynamic effects. In our subsequent event study estimates, we use all winning cohorts with all available later winners, holding the baseline pre-treatment year as two years pre-win. We produce these estimates using a sample (summarized in Appendix Table A.9) that begins with the universe of recipients of a Form W-2G with recorded state

[2]In the presence of possible anticipation of future lottery win, some of the year-on-year changes in the $E_i >$ 2003 group's outcome may be due to the anticipatory response in addition to pre-existing trends. We abstract from this in our discussion, but note that one natural approach to acknowledge the potential for such anticipation would be to allow for an anticipation window for later-winning cohorts and only use their observations in the period prior to the onset of anticipation. We take this anticipation window approach when producing event study estimates for the change in asset value and consumption expenditure, both of which include a first-difference term and hence mechnically includes something akin to "anticipation."

lottery income between 2001 and 2016. We first restrict attention to winners who also have recorded age and sex data available from SSA records. We then focus on individuals who were 21 to 64 years of age (i.e., working age) at the time of receiving the Form W-2G, and whose first recorded W-2G state lottery payment between 2001 and 2016 was for $30,000 or more. We use this first state lottery payment to define the size of the shock that an individual experienced; however, all individuals in the sample receive a lottery-induced income shock at some point between 2001 and 2016.

D.2 Lottery income shocks on earnings, employment, and related outcomes

We proceed to a discussion of our main findings on the effect of lottery income shocks on employment, earnings, and related outcomes. Starting with wage earnings of the winner, we find that cohorts shocked with lottery income decrease their earnings by approximately $4000 one year after winning, with relatively little change (increase or decrease) in this impact in the subsequent 4 years (Appendix Figure A.21). The analogous employment response is a 4 percentage point reduction in employment, with effects growing over time. Together with these responses in the labor market, individuals shocked with lottery income appear to increase their asset holdings as reflected by their increase in reported capital income (coming largely in the form of interest-bearing assets) and to increase their total expenditure (as implied by their chainge in capital income, once capitalized[3]). As individuals win lottery prizes of varying sizes, we normalize the above effects by scaling them by the mean post-tax adult-equivalent lottery winnings, constructing effects per-dollar won (post-tax) in Table A.11. In order to capture two potentially important sources of heterogeneity, we explore these per-dollar effects accomodating variation in patterns over time (short run[4] of years 1 and 2, and long run of years 3 to 5) and well as across pre-win incomes (quartiles of adjusted gross income). Starting, as before, from the labor market outcomes, we see that wage earnings decline by approximately $0.02 per post-tax dollar won. However, this masks heterogeneity in earnings responses across the income distribution – per-dollar effects increase (in absolute value) from $0.01 per post-tax dollar won up to $0.03 per post-tax dollar won as we move across income quartiles. This pattern of increasing (in absolute value) per dollar effects is reversed for employment effects (which we scale by 100,000 for readability). In addition, the per-dollar effects on consumption expenditure are declining in prize size, with a $0.035 increase in consumption expenditure per dollar won for the first income quartile winners, but a $0.026 increase in consumption expenditure per dollar won for the fourth quartile winers.

[3]For capitalizing capital income into asset value, we use the mean rate of return over time of 0.054, as calculated from the supplementary materials of Saez and Zucman (2016). We also explore alternative choices of capitalization rate; namely 0.07 and 0.10.

[4]As we do not observe precisely the date that an individual wins, we focus on effects starting from the first complete calendar year after the win year.

D.3 Heterogeneity in per-dollar effects by work history, prize size, and age

Work history

Our main (modest) effects on employment and earnings already suggest that our modeling assumptions may not be missing an important income effect response. However, we can more directly compare across our lottery analysis and our main analysis by focusing on individuals with relatively strong attachment to the labor force. In particular, throughout our main analysis we restrict attention to individuals that are full-time employed based on having labor earnings in excess of $15,000. To understand whether the pattern of earnings and employment responses to lottery income shocks is comparable in this strongly-attached group, we compare lottery-induced income effects when looking at the full sample to the effects for two subsamples: i) a subsample that was employed pre-win, and ii) a subsample that was employed pre-win and that received at least $15,000 in labor earnings prior to the lottery shock. The results of this comparison, summarized in Table A.12, suggest that greater labor force attachment is associated with a larger per-dollar effect on earnings (which is unsurprising when compared to the baseline sample which may have non-employed lottery winners). Nonetheless, the per-dollar effects are modest in size, and the pattern of increasing (in absolute value) per-dollar effects with pre-win income is preserved as we look at the subsamples of the data with greater labor force attachment.

Prize size

In the prior estimates, we collapse all lottery shocks into a single average lottery income shock. However, this aggregation masks potential heterogeneity across lottery shocks of different size. To proceed, we split the full sample into two subsamples. The first represents all lottery income shocks between $30,000 and $300,000, which we refer to as smaller winners. The second corresponds to lottery income shocks in excess of $1 million, which we refer to as larger winners. We summarize variation in per-dollar effects across these subsamples in Table A.13. Beginning with the employment effects (scaled by 100,000) we find that the response per dollar won on the extensive margin (work versus not) is declining (in absolute value) in prize size, and quite strongly. For earnings, we see a similar pattern of largely decreasing effects in win size. This dual pattern raises the question: is the decline in earnings effect by prize driven by earnings reductions among the continuing workers, or by employment reductions (i.e, the extensive margin). In Figure A.22 we conduct such a decomposition and find that close to two-thirds of the difference in earnings effect per dollar comes from the extensive margin.

Age

We consider the possibility that individuals at different parts of their lifecycle may respond differentially to an income shock of the same size. In particular, we focus our comparison in Figure A.24 to our baseline sample of 21-64 year olds versus a subsample achieving prime labor force participation (36-54). We find that the earnings effects per-dollar are broadly similar across age groups, both on average and across income quartiles. On the other hand, per-dollar effects on implied expenditure are lower for the prime age sample (roughly \$0.015 per dollar won) than for the full sample. However, both earnings and expenditure per dollar won maintain the pattern across income quartiles found in the baseline sample.

D.4 From per-dollar effects to per-period income effects

While lottery income shocks present a unique opportunity to study income effects, in order to make them more comparable to the kind of income shocks in our modelling approach, we need to translate the one-time wealth shock of a lottery prize into a change of per-period income. To do so, we take two approaches. In the first, we suppose that individuals live to the age of 80 and convert the one-time post-tax lottery win into a stream of fixed annuity payments (with an internal rate of return of 2.5%, approximately matching inflation-adjusted risk-free Treasury securities). In the second, we utilize observed sources of unearned income (such as dividends, interest payments, and rental and royalty income, among others) and use the capitalization approach (Saez and Zucman (2016)) to recover the winner's chosen allocation of unearned income over time. With both per-period income concepts, we form ratios as we did in the prior section, recovering per-period income effects for the set of outcomes explored before. In Table A.14 we summarize the per-period income effects when we take the annuitization approach. In particular, we can highlight that for each dollar of income from the lottery annuity, earnings fall by roughly \$0.48, and expenditure increases by \$0.68. As with the per-dollar effects, per-period income effects on earnings increase with pre-win income, whereas per-period income effects on expenditure decrease with pre-win income. In Table A.16, we instead use the second per-period income approach (allocated unearned income) and find that across outcomes of interest, per-period income effects are very similar across the two approaches to converting lottery income shocks into per-period income shocks, both in the full sample and across pre-win income quartiles. When we focus on the per-period income effects of those in the lottery sample more closely attached to the labor force (Table A.15; like those in our main analysis sample), we find that per-period income effects are larger for those that are more attached to the labor force, but, again, the pattern of effects increasing with pre-win income is preserved.

Finally, when we explore per-period income effects across prize size, we see that while our two approaches to converting lottery income shocks into per-period income shocks led to very similar results on average and by income, they lead to different results by prize size. In particular, whereas the per-period income effect on expenditure is decling in prize size when using the annuitization approach, the pattern is reversed when using allocated unearned income. Returning to Table A.11, we see why this the case: per-dollar allocation of income when annuitizing *underallocates* per-period income for small prizes, and *overallocates* per-period income for large prizes (relative to the allocated unearned income). While we do not find such a stark reversal for per-period income effects on earnings across prize, we do see a substantial dampening of per-period income effects when going from post-tax annuitized lottery proceeds to allocated unearned income.

D.5 Tables and Figures

Table A.9: Lottery Sample Summary

		Baseline Sample		All Tax Filers (21-64)
		Treated (Winners)	*Control (Not-Yet Winners)*	
Covariate	*Statistic*			
Wage Earnings	Mean	$34,598	$34,271	$33,005
Employment	Prop.	0.79	0.80	0.80
Female	Prop.	0.39	0.39	0.51
Age	Mean	43.93	41.84	41.78
Married	Prop.	0.45	0.45	0.58
Homeowner	Prop.	0.45	0.44	0.49
N		101,190		154,372,671

Notes: This table provides an overview of the state lottery sample that we use in our robustness check.

Figure A.18: Age-at-Win Distribution of Lottery Sample

Notes: This figures summarizes the age-at-win distribution in the lottery sample.

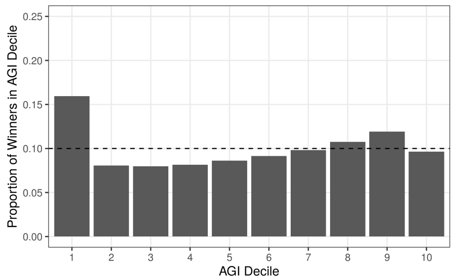
Table A.10: Distribution of Prizes

Prize Range	Frequency	Mean	Median	Annuitized Mean	Annuitized Median
< 2.5K annually	57,915	$30,424	$28,000	$1,347	$1,300
2.5K to 5K annually	21,207	$72,289	$73,900	$3,497	$3,400
5K to 10K annually	11,431	$137,315	$136,900	$6,940	$6,700
10K+ annually	10,637	$1,232,358	$368,500	$62,180	$19,500

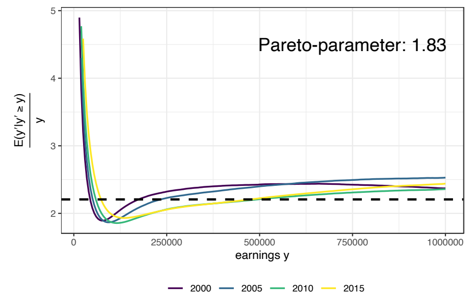
Notes: This table provides an overview of the distribution of prize winnings in the state lottery sample that we use in our robustness check. Medians rounded to nearest hundred.

Figure A.19: Features of Lottery Sample Income Distribution and Tax-Filer Income Distribution

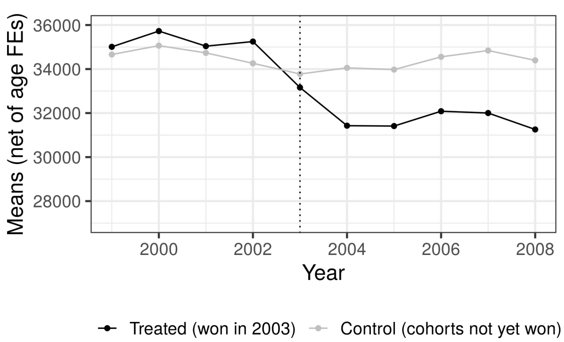
(a) Lottery Sample AGI Distribution Against Tax Filer Income Distribution



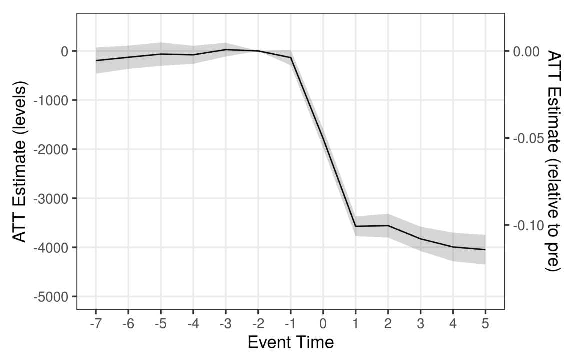
(b) Approximate Top Tail Parameter of Tax Filer Earnings Distribution



Notes: These figures summarize two features of relevant income distributions. Panel (a) compares the distribution of adjusted gross income (AGI) in the state lottery sample that we use in our robustness check to the population of tax filers on the basis of deciles of the tax filer income distribution. Panel (b) reports the top income tail parameter in the tax filer wage earnings distribution.



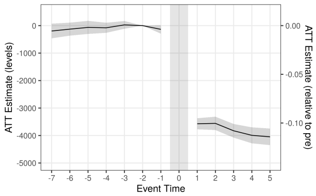
(c) 2003 Winners versus Later Winners (net of age effects)



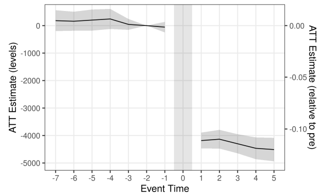
(d) Using All Possible Winners and Later Winners

Figure A.20: Motivation of Research Design (Wage Earnings)

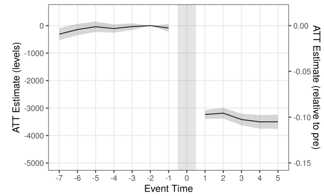
Notes: These figures help motivate the research design using later winners as a control group for current winners, using wage earnings as an example outcome. The top panel plots the mean wage earnings of those winning \$30,000+ in 2003 relative to those winning later, net of the contribution of age to earnings profiles. The bottom panel plots the estimated effect of the lottery shock on wage earnings combining all cohorts of winners and later winners. In the bottom panel, 90% confidence intervals are displayed, clustering on individual winners.



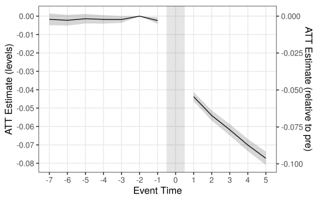
(a) Lottery-Induced Income Effect on Wage Earnings



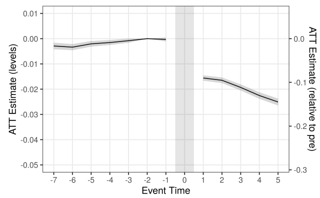
(b) Lottery-Induced Income Effect on Total Earnings (Wage + Self-Employment Income)



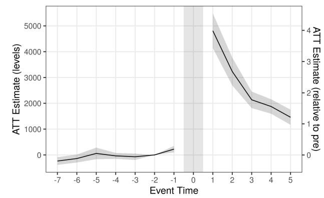
(c) Lottery-Induced Income Effect on Wage Earnings (Per-Adult)



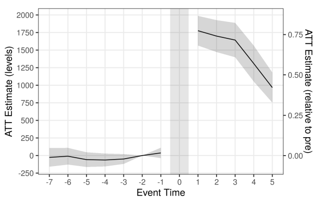
(d) Lottery-Induced Income Effect on Employment



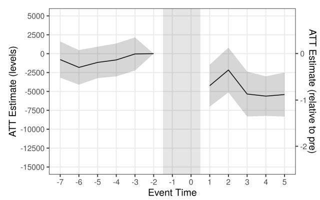
(e) Lottery-Induced Income Effect on Marginal Tax Rate



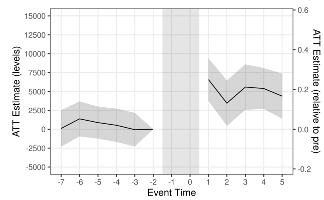
(f) Lottery-Induced Income Effect on Non-Wage Non-Capital Income



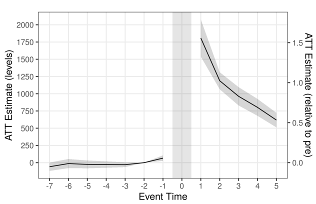
(g) Lottery-Induced Income Effect on Capital Income



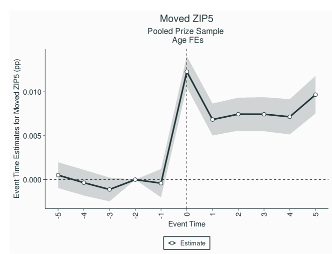
(h) Lottery-Induced Income Effect on ΔValue of Assets



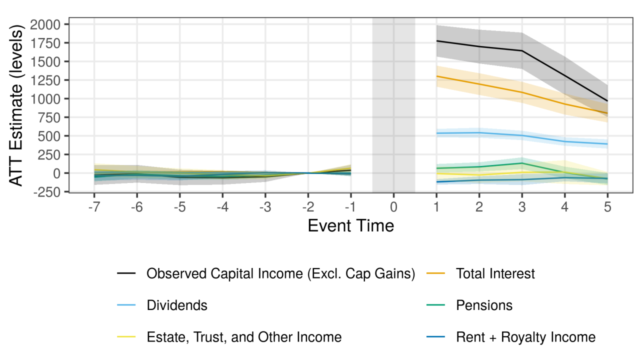
(i) Lottery-Induced Income Effect on Consumption Expenditure



(j) Lottery-Induced Income Effect on Total Taxes on Unearned Income



(k) Lottery-Induced Income Effect on Cross-Zip Code Moving Rate



(l) Components of Lottery-Induced Income Effect on Capital Income

Figure A.21: Responses to Lottery Shock

Notes: These figures present event study estimates of the effect of lottery-induced shocks on wage earnings and employment, as well as several outcomes which might have behavioral responses linked to earnings / employment responses. 90% confidence intervals are displayed, clustering on individual winners.

Table A.11: Per-Dollar-Won Effects

Outcome	Timing	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Full Sample
Wage Earnings	SR (+1 to +2)	-0.0103	-0.0183	-0.0208	-0.0272	-0.0197
	LR (+3 to +5)	-0.0139	-0.0217	-0.0249	-0.0297	-0.0224
	Avg (+1 to +5)	-0.0125	-0.0203	-0.0232	-0.0287	-0.0213
Total Earnings	SR (+1 to +2)	-0.0112	-0.0204	-0.0243	-0.0320	-0.0230
	LR (+3 to +5)	-0.0147	-0.0236	-0.0283	-0.0331	-0.0250
	Avg (+1 to +5)	-0.0133	-0.0223	-0.0266	-0.0327	-0.0242
Wage Earnings (per-adult)	SR (+1 to +2)	-0.0095	-0.0164	-0.0182	-0.0248	-0.0177
	LR (+3 to +5)	-0.0123	-0.0193	-0.0217	-0.0259	-0.0196
	Avg (+1 to +5)	-0.0112	-0.0181	-0.0202	-0.0254	-0.0189
Employment	SR (+1 to +2)	-0.0357	-0.0320	-0.0236	-0.0171	-0.0270
	LR (+3 to +5)	-0.0529	-0.0443	-0.0367	-0.0244	-0.0394
	Avg (+1 to +5)	-0.0461	-0.0393	-0.0312	-0.0215	-0.0344
Marginal Tax Rate	SR (+1 to +2)	-0.0048	-0.0127	-0.0067	-0.0070	-0.0089
	LR (+3 to +5)	-0.0104	-0.0143	-0.0111	-0.0101	-0.0126
	Avg (+1 to +5)	-0.0082	-0.0137	-0.0092	-0.0088	-0.0111
Capital Income	SR (+1 to +2)	0.0062	0.0084	0.0111	0.0115	0.0096
	LR (+3 to +5)	0.0050	0.0074	0.0084	0.0078	0.0074
	Avg (+1 to +5)	0.0055	0.0078	0.0095	0.0093	0.0083
Δ Value of Assets	SR (+1 to +2)	-0.0240	-0.0036	-0.0223	-0.0262	-0.0188
	LR (+3 to +5)	-0.0310	-0.0441	-0.0374	-0.0283	-0.0335
	Avg (+1 to +5)	-0.0281	-0.0278	-0.0310	-0.0275	-0.0274
Consumption Expenditure (CE)	SR (+1 to +2)	0.0386	0.0174	0.0309	0.0333	0.0293
	LR (+3 to +5)	0.0329	0.0434	0.0334	0.0213	0.0314
	Avg (+1 to +5)	0.0352	0.0330	0.0323	0.0262	0.0305
CE ($r = 0.07$)	SR (+1 to +2)	0.0322	0.0144	0.0229	0.0243	0.0229
	LR (+3 to +5)	0.0239	0.0308	0.0210	0.0099	0.0205
	Avg (+1 to +5)	0.0273	0.0242	0.0218	0.0158	0.0215
CE ($r = 0.10$)	SR (+1 to +2)	0.0267	0.0136	0.0178	0.0183	0.0185
	LR (+3 to +5)	0.0168	0.0207	0.0123	0.0034	0.0128
	Avg (+1 to +5)	0.0208	0.0178	0.0146	0.0095	0.0151
Allocated Unearned Income	SR (+1 to +2)	0.0461	0.0306	0.0457	0.0498	0.0426
	LR (+3 to +5)	0.0450	0.0606	0.0527	0.0404	0.0479
	Avg (+1 to +5)	0.0454	0.0485	0.0498	0.0442	0.0457
Post-Tax Annuitized Lottery Proceeds	SR (+1 to +2)	0.0433	0.0438	0.0448	0.0466	0.0448
	LR (+3 to +5)	0.0432	0.0438	0.0448	0.0462	0.0446
	Avg (+1 to +5)	0.0432	0.0438	0.0448	0.0463	0.0447

Notes: This table summarizes the event study estimates found in Figure A.21, but rescaling by the size of the lottery win (post-tax, and per-adult/equivalized), and also highlighting potential variation based on pre-win adjusted gross income. In general, i) using pre-tax lottery winnings and/or ii) not converting wins into per-adult (equivalized) values tend to reduce per-dollar-won effects (in absolute value). For example, average per-dollar-won effects on wage earnings using pre-tax non-equivalized lottery wins are -0.011, and average per-dollar-won effects on wage earnings using post-tax non-equivalized lottery wins are -0.016 in the full lottery sample. For readability, we scale the per-dollar effects of lottery winnings on marginal tax rates and employment by 100,000.

Table A.12: Variation in Average Per-Dollar-Won Effects by Labor Market Attachment

Outcome	Sample	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Full Sample
Wage Earnings	Baseline	-0.0125	-0.0203	-0.0232	-0.0287	-0.0213
	Employed Pre-Win	-0.0161	-0.0245	-0.0259	-0.0322	-0.0258
	FT Employed Pre-Win	-0.0218	-0.0272	-0.0268	-0.0331	-0.0287
Total Earnings	Baseline	-0.0133	-0.0223	-0.0266	-0.0327	-0.0242
	Employed Pre-Win	-0.0170	-0.0251	-0.0281	-0.0375	-0.0286
	FT Employed Pre-Win	-0.0251	-0.0280	-0.0290	-0.0380	-0.0317
Wage Earnings (per adult)	Baseline	-0.0112	-0.0181	-0.0202	-0.0254	-0.0189
	Employed Pre-Win	-0.0135	-0.0210	-0.0218	-0.0280	-0.0219
	FT Employed Pre-Win	-0.0162	-0.0230	-0.0222	-0.0282	-0.0241

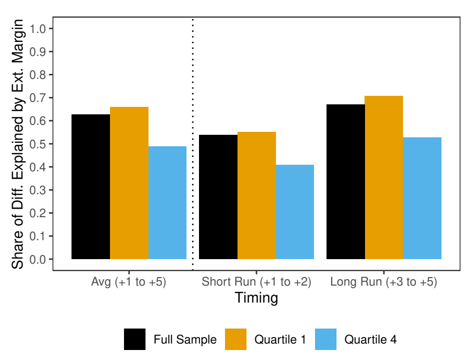
Notes: This table explores how the average earnings results in Table A.11 vary depending on labor market attachment prior to winning; in particular, exploring within a subsample that is employed (receives W-2 with non-zero gross wages) prior to winning, and a subsample that is employed and receiving full-time equivalent earnings prior to winning.

Table A.13: Variation in Per-Dollar-Won Effects by Prize Size

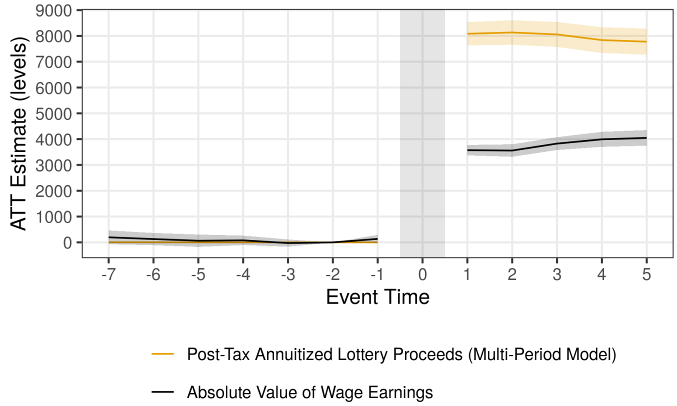
Outcome	Prize Size	Timing	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Full Sample
Wage Earnings	Smaller Prizes (30K - 300K)	SR (+1 to +2)	-0.0268	-0.0429	-0.0547	-0.0730	-0.0492
		LR (+3 to +5)	-0.0378	-0.0532	-0.0658	-0.0848	-0.0586
		Avg (+1 to +5)	-0.0334	-0.0491	-0.0613	-0.0801	-0.0549
	Larger Prizes (1M+)	SR (+1 to +2)	-0.0022	-0.0045	-0.0090	-0.0106	-0.0066
		LR (+3 to +5)	-0.0022	-0.0052	-0.0087	-0.0101	-0.0065
		Avg (+1 to +5)	-0.0022	-0.0049	-0.0088	-0.0103	-0.0065
Employment	Smaller Prizes (30K - 300K)	SR (+1 to +2)	-0.0938	-0.0712	-0.0619	-0.0440	-0.0701
		LR (+3 to +5)	-0.1474	-0.1064	-0.1001	-0.0684	-0.1086
		Avg (+1 to +5)	-0.1260	-0.0923	-0.0848	-0.0587	-0.0932
	Larger Prizes (1M+)	SR (+1 to +2)	-0.0074	-0.0078	-0.0101	-0.0068	-0.0081
		LR (+3 to +5)	-0.0071	-0.0097	-0.0117	-0.0085	-0.0093
		Avg (+1 to +5)	-0.0072	-0.0090	-0.0111	-0.0079	-0.0089
Consumption Expenditure (CE)	Smaller Prizes (30K - 300K)	SR (+1 to +2)	0.0851	0.0207	0.0588	0.0436	0.0476
		LR (+3 to +5)	0.0510	0.0166	0.0499	0.0319	0.0346
		Avg (+1 to +5)	0.0647	0.0182	0.0535	0.0366	0.0398
	Larger Prizes (1M+)	SR (+1 to +2)	0.0200	-0.0008	0.0348	0.0215	0.0188
		LR (+3 to +5)	0.0250	0.0419	0.0338	0.0271	0.0317
		Avg (+1 to +5)	0.0230	0.0254	0.0342	0.0250	0.0267
Allocated Unearned Income	Smaller Prizes (30K - 300K)	SR (+1 to +2)	0.1030	0.0480	0.0881	0.0701	0.0721
		LR (+3 to +5)	0.0818	0.0554	0.0897	0.0697	0.0694
		Avg (+1 to +5)	0.0903	0.0524	0.0891	0.0699	0.0705
	Larger Prizes (1M+)	SR (+1 to +2)	0.0189	0.0010	0.0387	0.0269	0.0214
		LR (+3 to +5)	0.0246	0.0454	0.0387	0.0331	0.0352
		Avg (+1 to +5)	0.0224	0.0282	0.0387	0.0307	0.0298
Post-Tax Annuitized Lottery Proceeds	Smaller Prizes (30K - 300K)	SR (+1 to +2)	0.0432	0.0434	0.0454	0.0470	0.0448
		LR (+3 to +5)	0.0432	0.0433	0.0452	0.0468	0.0446
		Avg (+1 to +5)	0.0432	0.0433	0.0453	0.0469	0.0447
	Larger Prizes (1M+)	SR (+1 to +2)	0.0436	0.0438	0.0451	0.0463	0.0447
		LR (+3 to +5)	0.0434	0.0434	0.0453	0.0459	0.0445
		Avg (+1 to +5)	0.0434	0.0435	0.0452	0.0461	0.0446

Notes: This table explores how results in Table A.11 vary across smaller winners (prize sizes between \$30,000 and \$300,000) and larger winners (prize sizes of \$1 million and above). For readability, we scale the per-dollar effects of lottery winnings on employment by 100,000.

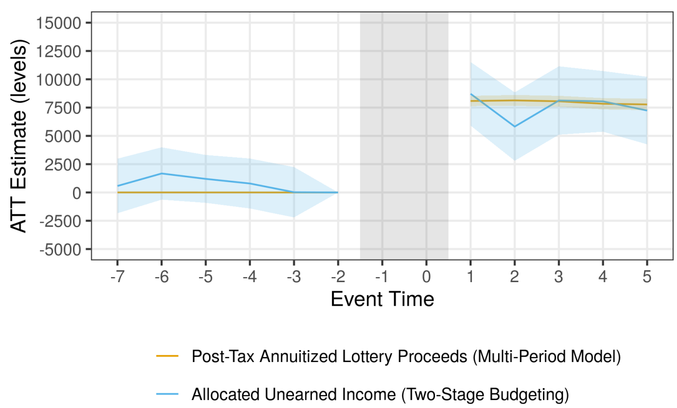
Figure A.22: Extensive Margin Share of Across-Prize Difference in Per-Dollar Earnings Effects



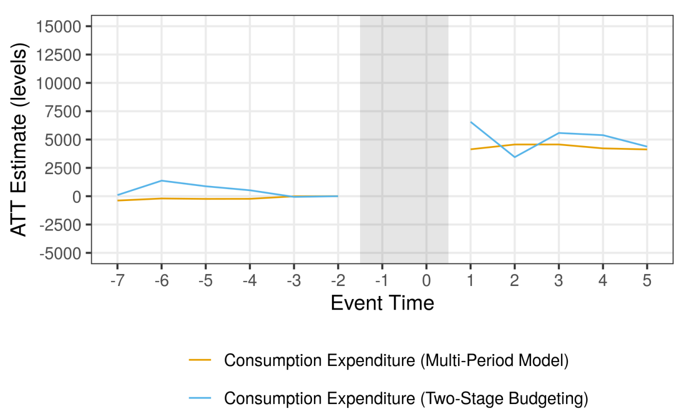
Notes: This figure plots the share of the difference in per-dollar earnings effects across prize size bins in Table A.13 that arises due to changes along the extensive (i.e., employment) margin.



(a) Graphical Motivation for Per-Annuitized-Dollar-Won Effects



(b) Per-Annuitized-Dollar versus Allocated Unearned Income



(c) Consumption Expenditure Implied by Annuitization versus Allocated Unearned Income

Figure A.23: Converting Lottery Shocks into Per-Period Income Shocks

Notes: These figures visualize how we recover the effect of an exogenous change in income from lottery shocks. Panel (a) illustrates graphically the ratio approach taken to form per-annuitized-dollar-won effects. Panel (b) compares one approach – annuitizing the post-tax lottery proceeds – to an alternative approach, using behavioral responses to lottery shocks in terms of capital income and other unearned income. Panel (c) compares the implied consumption expenditure under the two approaches to translating lottery shocks into per-period income shocks. 90% confidence intervals are displayed, clustering on individual winners.

Table A.14: Per-Annuitized-Dollar-Won Effects

Outcome	Timing	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Full Sample
Wage Earnings	SR (+1 to +2)	-0.2376	-0.4187	-0.4652	-0.5844	-0.4397
	LR (+3 to +5)	-0.3232	-0.4947	-0.5574	-0.6433	-0.5015
	Avg (+1 to +5)	-0.2891	-0.4639	-0.5189	-0.6195	-0.4764
Total Earnings	SR (+1 to +2)	-0.2598	-0.4666	-0.5429	-0.6876	-0.5130
	LR (+3 to +5)	-0.3413	-0.5397	-0.6313	-0.7173	-0.5610
	Avg (+1 to +5)	-0.3088	-0.5101	-0.5945	-0.7053	-0.5415
Wage Earnings (per-adult)	SR (+1 to +2)	-0.2191	-0.3742	-0.4058	-0.5319	-0.3959
	LR (+3 to +5)	-0.2850	-0.4410	-0.4844	-0.5607	-0.4403
	Avg (+1 to +5)	-0.2587	-0.4139	-0.4516	-0.5491	-0.4223
Employment	SR (+1 to +2)	-0.0082	-0.0073	-0.0053	-0.0037	-0.0060
	LR (+3 to +5)	-0.0123	-0.0101	-0.0082	-0.0053	-0.0088
	Avg (+1 to +5)	-0.0107	-0.0090	-0.0070	-0.0046	-0.0077
Marginal Tax Rate	SR (+1 to +2)	-0.0011	-0.0029	-0.0015	-0.0015	-0.0020
	LR (+3 to +5)	-0.0024	-0.0033	-0.0025	-0.0022	-0.0028
	Avg (+1 to +5)	-0.0019	-0.0031	-0.0021	-0.0019	-0.0025
Capital Income	SR (+1 to +2)	0.1440	0.1919	0.2473	0.2463	0.2143
	LR (+3 to +5)	0.1152	0.1694	0.1876	0.1698	0.1656
	Avg (+1 to +5)	0.1266	0.1785	0.2125	0.2008	0.1854
Δ Value of Assets	SR (+1 to +2)	-0.5496	-0.0824	-0.4981	-0.5620	-0.4174
	LR (+3 to +5)	-0.7134	-1.0054	-0.8343	-0.6096	-0.7483
	Avg (+1 to +5)	-0.6466	-0.6330	-0.6917	-0.5901	-0.6120
Consumption Expenditure (CE)	SR (+1 to +2)	0.8843	0.3953	0.6911	0.7121	0.6528
	LR (+3 to +5)	0.7566	0.9910	0.7444	0.4585	0.7007
	Avg (+1 to +5)	0.8087	0.7507	0.7218	0.5627	0.6810
CE ($r = 0.07$)	SR (+1 to +2)	0.7384	0.3269	0.5123	0.5213	0.5087
	LR (+3 to +5)	0.5509	0.7036	0.4671	0.2128	0.4573
	Avg (+1 to +5)	0.6274	0.5516	0.4863	0.3395	0.4785
CE ($r = 0.10$)	SR (+1 to +2)	0.6118	0.3080	0.3976	0.3918	0.4126
	LR (+3 to +5)	0.3866	0.4720	0.2749	0.0724	0.2850
	Avg (+1 to +5)	0.4784	0.4058	0.3269	0.2036	0.3375
Annual Lump Sum	SR (+1 to +2)	0.8398	0.8205	0.8424	0.7989	0.8211
	LR (+3 to +5)	0.8665	0.8209	0.8777	0.8933	0.8644
	Avg (+1 to +5)	0.8558	0.8207	0.8630	0.8551	0.8468
Unobserved Scaled Earnings Change	SR (+1 to +2)	-0.0170	-0.0795	-0.0765	-0.1385	-0.0928
	LR (+3 to +5)	-0.0440	-0.0940	-0.1163	-0.1889	-0.1283
	Avg (+1 to +5)	-0.0332	-0.0882	-0.1004	-0.1688	-0.1141

Notes: This table summarizes the event study estimates found in Figure A.21, but rescaling by the annuitized post-tax proceeds of the lottery winnings, and also highlighting potential variation based on pre-win adjusted gross income. For readability, we scale the annuitized per-dollar effects of lottery winnings on marginal tax rates and employment by 1,000. Similar to Table A.13, using pre-tax values and/or non-equivalizing prizes tends to reduce per-annuitized-dollar-won effects (in absolute value). Relatedly, note that individuals residing (at time of win) in states with differing state tax progressivity (or no state taxes) will have different annuitized post-tax proceeds from their lottery winnings holding age and nominal winnings fixed. In the above estimates, we implicitly average over this source of differences. For example, looking at those in the fourth quartile of pre-win income, we find that earnings effects per-annuitized-dollar-won are -0.4944 for winners residing states with no state income tax, but -0.8461 for winners residing in states with a flat state income tax, and -0.5963 for winners residing in states with progressive state income taxes. Along similar lines, annual lump sum per-annuitized-dollar-won and unobserved scaled earnings change per-annuitized-dollar-won are 0.7179 and -0.1047, respectively, in states with no state income tax, but 0.9235 and -0.2687, respectively, in states with flat state income taxes, and 0.8670 and -0.1786, respectively, in states with progressive state income taxes.

Table A.15: Variation in Average Per-Annuitized-Dollar-Won Effects by Labor Market Attachment

Outcome	Sample	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Full Sample
Wage Earnings	Baseline	-0.2891	-0.4639	-0.5189	-0.6195	-0.4764
	Employed Pre-Win	-0.3850	-0.5783	-0.5818	-0.6993	-0.5829
	FT Employed Pre-Win	-0.4929	-0.6471	-0.6045	-0.7196	-0.6446
Total Earnings	Baseline	-0.3088	-0.5101	-0.5945	-0.7053	-0.5415
	Employed Pre-Win	-0.4075	-0.5918	-0.6298	-0.8147	-0.6459
	FT Employed Pre-Win	-0.5666	-0.6662	-0.6528	-0.8259	-0.7116
Wage Earnings (per adult)	Baseline	-0.2587	-0.4139	-0.4516	-0.5491	-0.4223
	Employed Pre-Win	-0.3224	-0.4947	-0.4879	-0.6077	-0.4951
	FT Employed Pre-Win	-0.3658	-0.5482	-0.5013	-0.6134	-0.5399

Notes: This table explores how the average earnings results in Table A.14 vary depending on labor market attachment prior to winning; in particular, exploring within a subsample that is employed (receives W-2 with non-zero gross wages) prior to winning, and a subsample that is employed and receiving full-time equivalent earnings prior to winning.

Table A.16: Robustness to Alternative Allocation of Per-Period Income

Outcome	Timing	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Full Sample
Wage Earnings	SR (+1 to +2)	-0.2208	-0.6354	-0.5032	-0.5573	-0.4822
	LR (+3 to +5)	-0.3350	-0.3750	-0.5373	-0.7533	-0.4927
	Avg (+1 to +5)	-0.2879	-0.4410	-0.5240	-0.6629	-0.4887
Marginal Tax Rate	SR (+1 to +2)	-0.0010	-0.0042	-0.0016	-0.0015	-0.0022
	LR (+3 to +5)	-0.0024	-0.0023	-0.0023	-0.0028	-0.0028
	Avg (+1 to +5)	-0.0018	-0.0028	-0.0020	-0.0022	-0.0025
Consumption Expenditure (CE)	SR (+1 to +2)	0.8371	0.5693	0.6760	0.6677	0.6885
	LR (+3 to +5)	0.7310	0.7165	0.6332	0.5274	0.6553
	Avg (+1 to +5)	0.7748	0.6792	0.6499	0.5921	0.6680
Annual Lump Sum	SR (+1 to +2)	0.8493	0.7407	0.8370	0.8416	0.8193
	LR (+3 to +5)	0.8679	0.8702	0.8892	0.8837	0.8720
	Avg (+1 to +5)	0.8602	0.8374	0.8689	0.8643	0.8518
Unobserved Scaled Earnings Change	SR (+1 to +2)	-0.0170	-0.0795	-0.0765	-0.1385	-0.0928
	LR (+3 to +5)	-0.0440	-0.0940	-0.1163	-0.1889	-0.1283
	Avg (+1 to +5)	-0.0332	-0.0882	-0.1004	-0.1688	-0.1141

Notes: This table explores the robustness of results in Table A.14 when using allocated unearned income as the per-period income shock, rather than post-tax annuitized lottery proceeds. For readability, we scale the annuitized per-dollar effects of lottery winnings on marginal tax rates by 1,000.

Table A.17: Counterfactual Marginal Tax Rates

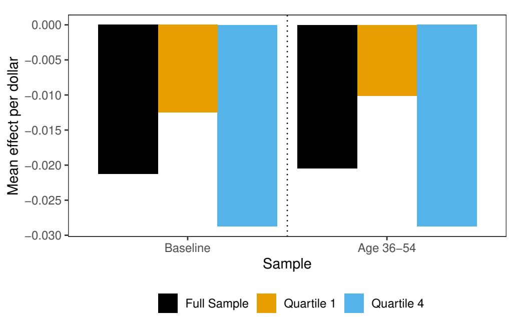
Per-Period Income Concept	Timing	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Full Sample
Post-Tax Annuitized Lottery Proceeds	SR (+1 to +2)	0.0267	0.1457	0.1994	0.2592	0.1686
	LR (+3 to +5)	0.0471	0.1392	0.1949	0.2520	0.1686
	Avg (+1 to +5)	0.0389	0.1418	0.1967	0.2549	0.1686
Allocated Unearned Income	SR (+1 to +2)	0.0257	0.1450	0.1990	0.2587	0.1683
	LR (+3 to +5)	0.0451	0.1381	0.1940	0.2511	0.1673
	Avg (+1 to +5)	0.0373	0.1409	0.1960	0.2541	0.1677

Notes: This table summarizes what the marginal tax rates among those winning $30,000+ would look like, on average, if, counterfactually, they had not won. Values vary across the two approaches to allocating per-period income as we lose one year of the lottery sample panel in the construction of allocated unearned income.

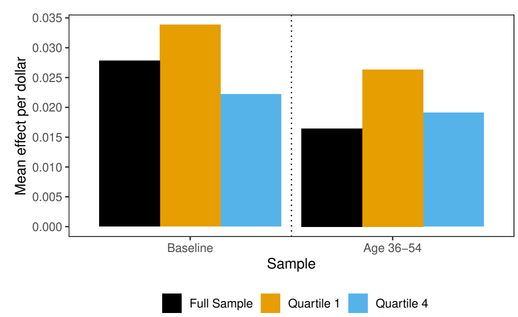
Table A.18: Variation in Per-Period Income Effects by Prize Size

Outcome	Prize Size	Timing	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Full Sample
Post-Tax Annuitized Lottery Proceeds							
Wage Earnings	Smaller Prizes (30K - 300K)	SR (+1 to +2)	-0.6198	-0.9893	-1.2045	-1.5536	-1.0984
		LR (+3 to +5)	-0.8756	-1.2297	-1.4558	-1.8110	-1.3133
		Avg (+1 to +5)	-0.7735	-1.1335	-1.3552	-1.7083	-1.2274
	Larger Prizes (1M+)	SR (+1 to +2)	-0.0495	-0.1031	-0.1989	-0.2283	-0.1474
		LR (+3 to +5)	-0.0509	-0.1188	-0.1921	-0.2202	-0.1464
		Avg (+1 to +5)	-0.0504	-0.1127	-0.1946	-0.2232	-0.1468
Consumption Expenditure (CE)	Smaller Prizes (30K - 300K)	SR (+1 to +2)	1.9676	0.4772	1.2961	0.9290	1.0635
		LR (+3 to +5)	1.1809	0.3843	1.1076	0.6825	0.7748
		Avg (+1 to +5)	1.4957	0.4216	1.1832	0.7813	0.8906
	Larger Prizes (1M+)	SR (+1 to +2)	0.4535	-0.0175	0.7744	0.4619	0.4190
		LR (+3 to +5)	0.5723	0.9586	0.7450	0.5846	0.7071
		Avg (+1 to +5)	0.5248	0.5782	0.7566	0.5377	0.5946
Allocated Unearned Income							
Wage Earnings	Smaller Prizes (30K - 300K)	SR (+1 to +2)	-0.2418	-0.9201	-0.6335	-0.9884	-0.6695
		LR (+3 to +5)	-0.4723	-0.9807	-0.7399	-1.1169	-0.8209
		Avg (+1 to +5)	-0.3671	-0.9585	-0.6978	-1.0654	-0.7589
	Larger Prizes (1M+)	SR (+1 to +2)	-0.1189	-4.6974	-0.2468	-0.4104	-0.3225
		LR (+3 to +5)	-0.0972	-0.1160	-0.2613	-0.3226	-0.1989
		Avg (+1 to +5)	-0.1045	-0.1795	-0.2556	-0.3519	-0.2334
Consumption Expenditure (CE)	Smaller Prizes (30K - 300K)	SR (+1 to +2)	0.8265	0.4316	0.6673	0.6220	0.6604
		LR (+3 to +5)	0.6240	0.2997	0.5564	0.4574	0.4980
		Avg (+1 to +5)	0.7164	0.3480	0.6002	0.5234	0.5644
	Larger Prizes (1M+)	SR (+1 to +2)	1.0579	-0.7640	0.8984	0.7981	0.8815
		LR (+3 to +5)	1.0136	0.9236	0.8732	0.8201	0.9005
		Avg (+1 to +5)	1.0285	0.9002	0.8832	0.8127	0.8952

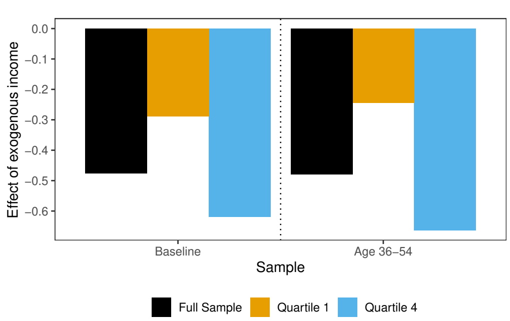
Notes: This table explores how results in Table A.14 vary across smaller winners (prize sizes between $30,000 and $300,000) and larger winners (prize sizes of $1 million and above), and also depending on how we define the per-period income shock (post-tax annuitized lottery proceeds or allocated unearned income).



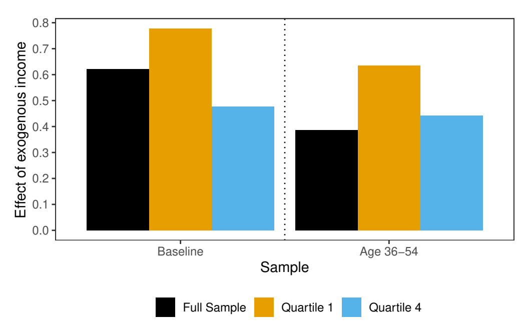
(a) Full Sample versus Prime Age (Per-Dollar Won; Wage Earnings)



(b) Full Sample versus Prime Age (Per-Dollar Won; Consumption Expenditure)



(c) Full Sample versus Prime Age (Per-Annuitized-Dollar; Wage Earnings)



(d) Full Sample versus Prime Age (Per-Annuitized Dollar; Consumption Expenditure)

Figure A.24: Heterogeneity in Effects by Age

Notes: These figures compare per-dollar / per-annuitized dollar effects on wage earnings and consumption expenditure in the full baseline sample to a sample of prime-age (36-54) winners.

E Appendix: Analyses based on Employment Shocks from Firm Entry and Exit

E.1 Explanation of the Empirical Approach

Consider the following regression equation:

$$\log y_{i,t} - \log y_{i,t-1} = \beta_0 + \beta_1 X_{cz(i),t} + \gamma K_{i,t} + \epsilon_{i,t}, \tag{15}$$

where i is the worker or firm, y is an outcome such as the log earnings, $cz(i)$ is its commuting zone, $X_{cz,t}$ denotes the *growth* in the employment share by foreign-owned firms in that commuting zone, and $K_{i,t}$ is a vector of controls such as industry-year fixed effects, CZ fixed effects, and a polynomial in age. The parameter of interest is β_1, which captures how the entry of a foreign-owned firm affects a worker outcome or a firm outcome at a non-foreign-owned firm in the same commuting zone. We will refer to β_1 as the "indirect effect" of firm entry. The de-identified tax records linking workers to firms analyzed here allow for the construction of each of these variables, using the filing of Form 5472 as an indicator for foreign-ownership status. Basic descriptive statistics on the counts and outcome means of foreign-owned and non-foreign-owned firms for the 2015 cross-section are presented in Appendix Table A.19. Appendix Figure A.25 plots how foreign-owned and non-foreign-owned firms differ over time in their value added and earnings, with and without residualizing on industry-year, commuting zone-year, and a polynomial in log size of the firm. When aggregating the number and share of employees at foreign-owned firms nationally over time, this data matches well the national trends reported by the BEA, as demonstrated in Appendix Figure A.26.

To identify β_1, we adapt the identification strategy common in the literature about the effects of immigration on non-immigrants in the same region (see Card, 2001). This literature uses the fact that immigrants cluster into regions in the US based on country of origin. To adapt this instrument to identify the effects of foreign-owned firm entry on workers, we first notice that employment at foreign-owned firms tends to be clustered by region and country of origin. For example, German-owned firms disproportionately employ workers in South Carolina in 2010 if they do so in 2005. This is analogous to the clustering of immigrants into regions. We construct a "Bartik instrument" as the predicted change in employment at, for example, German-owned firms in South Carolina between 2009 and 2010 using only information about (i) the share of workers at German-owned firms in South Carolina in 2005, and (ii) the change in aggregate employment by German-owned firms in *any other* region in the US between 2009 and 2010. Since this instrument is not formed using information about the change in employment by German-owned firms in South Carolina between 2009 and 2010, it does not depend directly on changes in South Carolina's business climate between 2009 and 2010. For example, it does not depend directly on infrastructure investments, improved educational opportunities, or changes in the generosity of tax incentives in South Carolina between 2009 and 2010. In Appendix Figure A.27, we provide evidence that ownership shares vary substantially over time and that spatial clustering occurs by nationality.[5]

Formally the identification challenge arises because, for example, the unobserved component of earnings growth $\epsilon_{i,t}$ (e.g., changes in the local demand for labor) may be correlated with $X_{cz,t}$. In order to form the instrument, we use the tax data on the firm's country of foreign ownership to construct the share $S^o_{cz,t}$ of all employment in commuting zone cz at firms whose owners are located in origin country o, defined by,

$$S^o_{cz,t} \equiv \frac{N^{F_o}_{cz,t}}{\sum_{cz'} N^{F_o}_{cz',t}} \tag{16}$$

Analogous to Card, 2001 and the subsequent immigration literature, we then construct the instrumental variable $Z_{cz,t}$ as,

$$Z_{cz,t} = \frac{\sum_o (\sum_{cz' \neq cz} N^{F_o}_{cz',t} - N^{F_o}_{cz',t-1}) S^o_{cz,t-5}}{N^F_{cz,t-5} + N^N_{cz,t-5}} \tag{17}$$

This is interpreted as the prediction of $X_{cz,t}$, formed only from the share of employment by firms from country o in cz dated at $t-5$ and the change in aggregate employment by o in the US from $t-1$ to t. Note that we modify the approach from the immigration literature slightly by leaving out own-commuting zone employment when constructing the aggregate change from $t-1$ to t, which helps to rule out confounding factors. The denominator is the total number of FTE workers in the commuting zone 5 years ago. Because $Z_{cz,t}$ is not a function of cz-specific changes between $t-1$ and t, it should satisfy that $Z_{cz,t}$ and the unexplained component of earnings growth are orthogonal (conditional on observed covariates that explain earnings growth). However, we see three major threats to identification, which we discuss below.

First, the instruments include the past share of employment at foreign-owned firms from various source countries as well as the change in the employment at such firms in other regions. This raises the concern that there may be regional shocks that are correlated with our instrument. For example, regions near the Canadian border may be affected also by trade shocks originating in Canada, that are correlated with the instrument. To deal with this concern, we include region-year fixed effects (where we define region as Census Division) in the regressions, that absorb all contemporaneous effects at the regional level.

Second, recent work by Jaeger et al., 2018 suggests that in the context of immigration past share of immigrants could have a direct effect on contemporaneous outcomes, if the adjustment to former immigrant waves is delayed. The analogous concern in our setting is that adjustment to past investment by foreign-owned firms is ongoing. Since our instrument leverages variation in the country of origin of foreign-owned firm investment across commuting zones, we can include as a control variable the share of past employment at foreign-owned firms (not separated by country of origin) in the commuting zone. We provide robustness results to our main regressions when also including the share of employment at foreign-owned firms in $t-5$ as a control variable. We

[5]Spatial clustering of foreign investment by nationality in the US has also been recognized by Burchardi et al., 2016. They find that the stock of past migrants in a county from a certain origin country can help to predict today's foreign investment in the county by firms from that origin country.

find that our main results are quantitatively robust to adding this control.

The third threat to identification is that industry shocks may be correlated with the instrument. For example, German- or Japanese-owned firms may be more likely to be in the car industry and select commuting zones that are also abundant with other car industry firms. To deal with this concern, we also include fine industry-year fixed effects that absorb any contemporaneous nation-wide growth trends by industry. Furthermore, note that by including CZ-fixed effects in our first difference specification, we control for a CZ-specific linear time trend in the outcome variable.

In Appendix E.2 below, we present and discuss the parameter estimates and the results from a number of specifications and robustness checks. As shown in Appendix Table A.21, we find positive and statistically significant effects of a firm entry shock on existing firms for hiring of new employees, total wage bill growth within the firm, and value added growth. To put the estimates in context, we find that, if a firm employing 10% of employees in the commuting zone exits and lays off its workers, then workers in existing employment relationships at other firms in the same commuting zone experience a 4.5% decline in employment, a 4.7% decline in earnings payments to workers, and a 6.4% decline in the firm's value added. The effects are especially concentrated in large firms and in firms in the tradeables sector. Appendix Table A.22 demonstrates that the results are broadly similar when considering the controls and alternate samples motivated by the potential identification threats discussed above, and Appendix Table A.23 demonstrates that the effects are strongest for industries that are. As shown in Appendix Table A.20, we find a positive and statistically significant effect of a firm entering the market on the earnings of continuing workers at existing firms in the same commuting zone among the top quintile group, whose average annual earnings are $156,700, and the second highest quintile group, whose average annual earnings are $63,200.

Finally, in Appendix Table A.25, we consider effects of firm entry or exit on household income and tax outcomes for workers at other firms. To interpret the results, if a firm employing 10% of employees in the commuting zone exits and lays off its workers, then workers at existing firms in the same commuting zone experience a 4.5% decrease in household income, 4.0% decrease in net household income, and a 6.8% decrease in tax payments. Respectively, these results suggest that household responses provide little insurance against these shocks, the Federal tax-and-transfer system provides about a 9% rate of insurance against these shocks, and tax payments are more sensitive to these shocks than wages, which is consistent with a progressive tax system. Finally, we find that the layoff shock leads workers in other firms to claim EITC at a higher rate (extensive margin of EITC take-up) and to claim higher EITC deductions (intensive margin of EITC take-up).

In order to better understand the mechanisms through which these firm entry shocks are passed from foreign-owned firms to other firms in the same commuting zone, we investigate the role of workers moving between these two types of firms as a potential transmission mechanism. First, Appendix Table A.24 shows the differences in mean log earnings, mean firm effects, and mean worker effects provided to the workers at the foreign-owned firms relative to non-foreign-owned firms, using the two-way fixed effects estimates from the movers analyses presented in

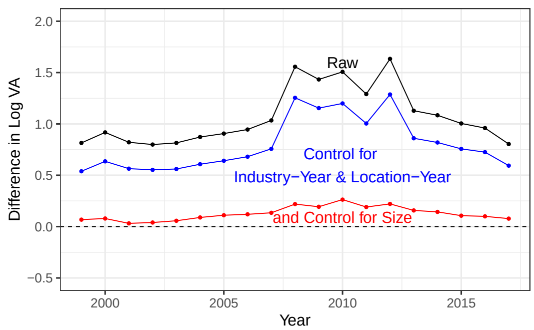
Section 4.2. It presents the results both overall and conditioning on firms of approximately the same size. The higher firm premiums (7.2%) and higher worker quality (12.7%) at these firms, as well as the noticeably higher firm premiums to higher-skilled workers, are consistent with these firms having higher average productivity and stronger skill-augmenting technology. Since 87.1% of new hires at foreign firms from domestic firms are from the same commuting zone, these firm premiums are primarily paid to incumbents in the region. Furthermore, Appendix Table A.26 provides estimates for the largest origin countries in the sample, finding evidence that distance serves as a cost to entry so that average productivity is higher when the country of origin is further away. Second, as illustrated in Appendix Figure A.28 and supported by the regression coefficients in Appendix Table A.27, workers see an increase in wages when moving from non-foreign-owned to foreign-owned firms. This is true for raw and residual earnings measures, and it is true for the full sample as well as the sample of workers whose firms experienced a layoff with 30% or more of workers moving. While these findings are only suggestive, they are consistent with technological spillovers hypothesized in the literature on firm entry, in which workers (especially higher-skilled workers) learn new technological processes from new high-productivity firms and later communicate this technology to other firms in the same labor market.

E.2 Data Description and Results on the Effects of Firm Entry and Exit

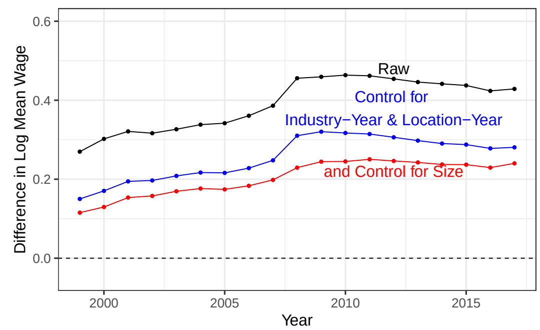
	Domestic	Foreign
Firms in Main Sample of Firms (thousands)	2,781.1	30.3
Firm-Location Pairs in Main Sample of Firms (thousands)	4,762.9	218.7
Number of Workers at Main Sample of Firms (millions):		
All Workers:	77.1	5.2
FTE Analysis Sample:	41.3	3.6
Mean Wage at Main Sample of Firms (thousands):		
All Workers:	41.4	60.7
FTE Analysis Sample:	62.6	75.7
Value Added per Worker at Main Sample of Firms (thousands):		
All Workers:	82.7	153.1
FTE Analysis Sample:	154.3	220.1
Sample Exit Rates among those Active in 2014:		
Workers:	0.271	0.217
Firms:	0.123	0.137

Table A.19: 2015 Cross-sectional Descriptive Statistics on the Samples used in the Firm Entry Analysis

Notes: This table displays descriptive statistics for domestic-owned and foreign-owned firms that file forms 1120, 1120-S, and 1065, matched to subsidiaries and W-2 forms.



(a) Mean Difference in Log Value Added



(b) Mean Difference in Log Wage bill per worker

Figure A.25: Changes over Time in Differences between Foreign-owned and Non-foreign-owned Firms

Notes: This figure presents mean differences over time between foreign-owned and domestic-owned firms in log value added and log wage bill per worker. It presents these differences for the raw measures as well as the residuals from regressions on industry-year, commuting zone-year, and a polynomial in log firm size.

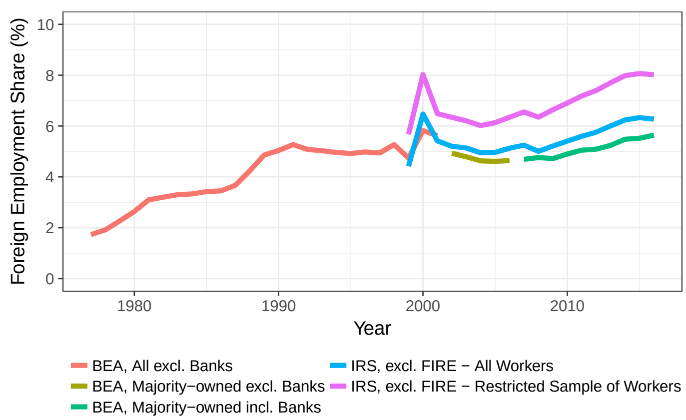
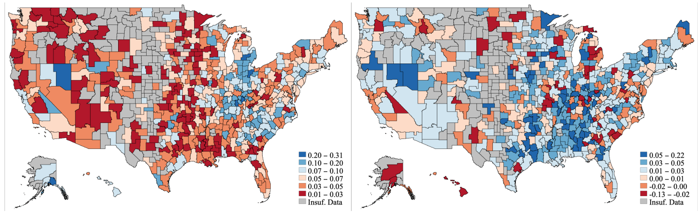
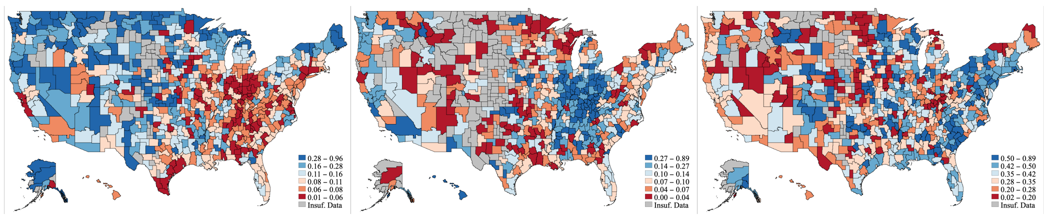


Figure A.26: Validating that Foreign Ownership is Measured Similarly to BEA

Notes: This figure compares the total employment counts and shares at foreign-owned firms over time observed in tax records to those reported by the BEA, where BEA has provided three distinct series of measurements over time.



(a) Any Owner Country, 2001 (b) Any Owner Country, 2001-2015 Change



(c) Canadian Owned (d) East Asian Owned (e) Western European Owned

Figure A.27: Geographic Concentration and Changes in Employment Shares at Foreign-owned Firms

Notes: This figure compares shares of employment at foreign-owned firms for all countries of ownership in 2001 and the change from 2001 to 2015, as well as disaggregating by groups of countries of ownership and averaging across all years.

		By Income Quintile Group				
	Full Sample	**Quintile 1**	**Quintile 2**	**Quintile 3**	**Quintile 4**	**Quintile 5**
	Outcome: Log Wage (continuing workers)					
2SLS Indirect Effect	**0.067**	**-0.086**	**-0.038**	**0.016**	**0.192****	**0.292*****
	(0.063)	(0.074)	(0.062)	(0.066)	(0.081)	(0.092)
First Stage Coefficient	0.599***	0.595***	0.594***	0.598***	0.595***	0.599***
	(0.035)	(0.036)	(0.035)	(0.035)	(0.035)	(0.036)
First Stage F-statistic	301	280	282	288	295	280
Worker Observations (Millions)	369.6	73.9	73.9	73.9	73.9	73.9

Table A.20: Worker-level Bartik IV Estimates: Indirect Effects of Firm Entry on Continuing Workers at Non-foreign-owned Firms in the Same Commuting Zone

Notes: Controls are 3-digit-industry-year fixed effects, Census Division-year fixed effects, CZ fixed effects, and a polynomial in worker age. Recall that firm fixed effects are removed through the first-differenced specification. Standard errors are clustered at the CZ-year level. The sample only includes continuing workers at domestic-owned firms. We divide workers into five wage quintile groups within each CZ-year based on the ordering of their lagged wages.

	Full Sample			2SLS by Firm Size			2SLS by Sector	
	OLS	OLS+controls	2SLS	Size 1-9	Size 10-99	Size 100+	Tradables	Non-tradables
Panel A.					**Outcome: Log Value Added**			
2SLS Indirect Effect	**-1.206*****	**0.323****	**0.644****	**0.106**	**0.416*****	**1.662***	**3.375***	**0.311**
			(0.266)	(0.080)	(0.147)	(0.985)	(1.985)	(0.193)
First Stage Coefficient			0.598***	0.634***	0.585***	0.526***	0.559***	0.524***
			(0.035)	(0.031)	(0.034)	(0.043)	(0.043)	(0.037)
First Stage F-statistic			299	431	292	147	169	197
Firm Observations (Millions)	41.7	41.7	41.7	34.9	6.3	0.5	3.9	5.9
Panel B.					**Outcome: Log Full-time Workers**			
2SLS Indirect Effect	**-0.072**	**0.167*****	**0.446*****	**0.077**	**0.387*****	**1.234*****	**0.888****	**0.544*****
			(0.125)	(0.065)	(0.135)	(0.429)	(0.384)	(0.203)
First Stage Coefficient			0.598***	0.635***	0.583***	0.534***	0.562***	0.522***
			(0.035)	(0.030)	(0.034)	(0.043)	(0.043)	(0.038)
First Stage F-statistic			297	434	292	151	171	192
Firm Observations (Millions)	45.9	45.9	45.9	38.3	7.0	0.5	4.2	6.2
Panel C.					**Outcome: Log Wage Bill**			
2SLS Indirect Effect	**-0.598*****	**0.228*****	**0.466*****	**0.025**	**0.368****	**1.154*****	**0.890****	**0.895*****
			(0.138)	(0.088)	(0.156)	(0.424)	(0.405)	(0.279)
First Stage Coefficient			0.598***	0.635***	0.583***	0.534***	0.562***	0.522***
			(0.035)	(0.030)	(0.034)	(0.043)	(0.043)	(0.038)
First Stage F-statistic			297	434	292	151	171	192
Firm Observations (Millions)	45.9	45.9	45.9	38.3	7.0	0.5	4.2	6.2

Table A.21: Firm-level Bartik IV Estimates: Main Effects on All Other Firms

Notes: Controls are 3-digit-industry-year fixed effects, Census Divison-year fixed effects, CZ fixed effects, and a polynomial in $t-1$ firm size. Recall that firm fixed effects are removed through the first-differenced specification. Standard errors are clustered at the CZ-year level. Observations are weighted by lagged firm size. The sample only includes continuing domestic-owned firms.

	Baseline	6-digit NAICS Fixed Effects	Lagged FDI as a Control	Exclude Multinationals	Exclude 250m Radius from Z	Positive Premium	Restricted FDI Origins
Panel A.				**Outcome: Log Value Added**			
2SLS Indirect Effect	**0.644****	**0.712*****	**0.629****	**0.579*****	**0.610****	**0.590****	**0.670****
	(0.266)	(0.220)	(0.268)	(0.221)	(0.286)	(0.266)	(0.295)
First Stage Coefficient	0.598***	0.596***	0.591***	0.612***	0.647***	0.617***	0.574***
	(0.035)	(0.034)	(0.035)	(0.034)	(0.046)	(0.036)	(0.035)
First Stage F-statistic	299	300	291	333	196	293	268
Firm Observations (Millions)	41.7	41.7	41.7	40.4	41.7	41.7	41.7
Panel B.				**Outcome: Log Full-time Workers**			
2SLS Indirect Effect	**0.446*****	**0.434*****	**0.441*****	**0.410*****	**0.449*****	**0.415*****	**0.457*****
	(0.125)	(0.120)	(0.125)	(0.120)	(0.134)	(0.124)	(0.138)
First Stage Coefficient	0.598***	0.597***	0.592***	0.609***	0.648***	0.620***	0.574***
	(0.035)	(0.035)	(0.035)	(0.034)	(0.046)	(0.037)	(0.035)
First Stage F-statistic	297	298	289	325	195	283	264
Firm Observations (Millions)	45.9	45.9	45.9	44.5	45.9	45.9	45.9
Panel C.				**Outcome: Log Wage Bill**			
2SLS Indirect Effect	**0.466*****	**0.457*****	**0.453*****	**0.455*****	**0.477*****	**0.393*****	**0.487*****
	(0.138)	(0.137)	(0.138)	(0.140)	(0.151)	(0.137)	(0.152)
First Stage Coefficient	0.598***	0.597***	0.592***	0.609***	0.648***	0.620***	0.574***
	(0.035)	(0.035)	(0.035)	(0.034)	(0.046)	(0.037)	(0.035)
First Stage F-statistic	297	298	289	325	195	283	264
Firm Observations (Millions)	45.9	45.9	45.9	44.5	45.9	45.9	45.9

Table A.22: Firm-level Bartik IV Estimates: Robustness Checks

Notes: Unless otherwise specified in the column header, controls are 3-digit-industry-year fixed effects, Census Divison-year fixed effects, CZ fixed effects, and a polynomial in $t-1$ firm size. Recall that firm fixed effects are removed through the first-differenced specification. Standard errors are clustered at the CZ-year level. Observations are weighted by lagged firm size. The sample only includes continuing non-foreign-owned firms. The robustness checks are, in order: including finer fixed effects for industry using the 6-digit NAICS code; including the total lagged FDI intensity in the region as a control variable; excluding firms with positive foreign tax payments; excluding FDI in any commuting zone within a 250 mile radius from the "shift" component of the Bartik instrument; including only firms from origin countries with positive estimates of the average firm premium relative to the domestic average; and restricting FDI origins to avoid potential misclassification.

	Outcome: Log Value Added		
	Effect of FDI Growth on Horizontal Domestic Firms	Effect of FDI Growth on Upstream Domestic Firms	Effect of FDI Growth on Downstream Domestic Firms
2SLS Indirect Effect	**0.133**	**-0.574**	**0.410**
	(0.118)	(0.417)	(0.326)
First Stage F-statistic (3 IVs)	108	13	21
Firm Observations (Millions)		37.0	

	Outcome: Log Full-time Workers		
	Effect of FDI Growth on Horizontal Domestic Firms	Effect of FDI Growth on Upstream Domestic Firms	Effect of FDI Growth on Downstream Domestic Firms
2SLS Indirect Effect	**0.038**	**0.235**	**0.389****
	(0.063)	(0.218)	(0.172)
First Stage F-statistic (3 IVs)	109	13	23
Firm Observations (Millions)		40.0	

	Outcome: Log Wage Bill		
	Effect of FDI Growth on Horizontal Domestic Firms	Effect of FDI Growth on Upstream Domestic Firms	Effect of FDI Growth on Downstream Domestic Firms
2SLS Indirect Effect	**0.061**	**-0.060**	**0.352***
	(0.076)	(0.306)	(0.205)
First Stage F-statistic (3 IVs)	109	13	23
Firm Observations (Millions)		40.0	

Table A.23: Firm-level Bartik IV Estimates: Industry Decomposition

Notes: Unless otherwise specified in the column header, controls are 3-digit-industry-year fixed effects, Census Divison-year fixed effects, CZ fixed effects, and a polynomial in $t-1$ firm size. Recall that firm fixed effects are removed through the first-differenced specification. Standard errors are clustered at the CZ-year level. Observations are weighted by lagged firm size. It examines whether effects are on other firms in the horizontal, upstream, or downstream industry.

Size Bin:	All	-2	-1.5	-1	-0.5	0	0.5	1	1.5	2	2.5	3	3.5	4	4.5	5
Full Sample:																
Share Foreign Firms	0.013	0.014	0.012	0.008	0.010	0.010	0.011	0.014	0.017	0.023	0.030	0.041	0.053	0.068	0.085	0.104
Log Value Added	0.755	0.033	-0.241	-0.032	-0.016	0.078	0.162	0.223	0.263	0.269	0.283	0.331	0.340	0.418	0.417	0.438
Log Mean Wage bill	0.250	0.436	0.167	0.243	0.264	0.255	0.236	0.202	0.164	0.132	0.102	0.102	0.108	0.111	0.083	0.090
Connected Set 2010-15: Baseline																
Log Earnings	0.194	0.355	0.257	0.294	0.242	0.223	0.193	0.155	0.134	0.105	0.078	0.088	0.081	0.096	0.066	0.084
Firm Effect	0.072	0.125	0.090	0.105	0.087	0.080	0.067	0.053	0.049	0.042	0.035	0.040	0.038	0.044	0.036	0.040
Worker Effect	0.127	0.234	0.177	0.200	0.164	0.151	0.134	0.109	0.091	0.068	0.047	0.050	0.046	0.053	0.032	0.046
Connected Set 2001-2006: Baseline																
Log Earnings	0.170	0.263	0.201	0.236	0.216	0.194	0.172	0.143	0.111	0.101	0.083	0.071	0.077	0.062	0.053	0.084
Firm Effect	0.067	0.096	0.073	0.084	0.079	0.072	0.066	0.056	0.046	0.046	0.041	0.039	0.041	0.033	0.033	0.046
Worker Effect	0.105	0.171	0.130	0.154	0.139	0.125	0.110	0.091	0.069	0.057	0.046	0.034	0.038	0.031	0.023	0.039
Connected Set 2010-15: Size controls																
Log Earnings	0.192	0.386	0.281	0.312	0.266	0.246	0.217	0.180	0.159	0.131	0.104	0.114	0.107	0.119	0.086	0.109
Firm Effect	0.063	0.108	0.081	0.094	0.082	0.077	0.067	0.058	0.056	0.051	0.045	0.053	0.050	0.056	0.046	0.052
Worker Effect	0.131	0.268	0.201	0.221	0.185	0.170	0.150	0.123	0.105	0.081	0.060	0.061	0.057	0.062	0.042	0.057
Connected Set 2010-15: Allowing 20 clusters																
Firm Effect	0.071	0.132	0.093	0.108	0.088	0.080	0.067	0.052	0.047	0.039	0.034	0.038	0.038	0.045	0.035	0.037
Worker Effect	0.129	0.228	0.175	0.199	0.164	0.151	0.134	0.110	0.094	0.071	0.048	0.052	0.045	0.052	0.033	0.048
Connected Set 2010-15: No bias correction																
Firm Effect	-0.015	0.073	-0.028	-0.009	-0.015	-0.016	-0.028	-0.041	-0.052	-0.050	-0.050	-0.032	-0.036	-0.010	-0.022	-0.020
Worker Effect	0.210	0.282	0.285	0.303	0.258	0.239	0.222	0.196	0.186	0.155	0.128	0.120	0.117	0.106	0.088	0.103
Connected Set 2010-15: Firm-worker interactions																
Worker Effect	0.117	0.227	0.167	0.191	0.154	0.139	0.120	0.097	0.078	0.058	0.040	0.043	0.040	0.046	0.029	0.042
Firm Effect Mean	0.078	0.127	0.091	0.109	0.091	0.085	0.071	0.057	0.053	0.042	0.035	0.040	0.040	0.045	0.038	0.040
Firm Effect Quantiles:																
10	0.006	0.040	0.010	0.022	0.012	0.006	-0.002	-0.009	-0.009	-0.010	-0.005	0.001	0.002	0.005	-0.004	-0.002
20	0.027	0.065	0.034	0.048	0.036	0.029	0.019	0.010	0.009	0.006	0.007	0.012	0.014	0.017	0.008	0.010
30	0.043	0.085	0.052	0.068	0.053	0.047	0.036	0.025	0.023	0.017	0.016	0.021	0.022	0.026	0.018	0.020
40	0.058	0.103	0.069	0.085	0.069	0.063	0.051	0.039	0.036	0.028	0.024	0.029	0.030	0.034	0.026	0.028
50	0.072	0.120	0.085	0.103	0.085	0.078	0.065	0.052	0.048	0.038	0.032	0.037	0.037	0.042	0.035	0.036
60	0.087	0.138	0.101	0.120	0.101	0.095	0.080	0.065	0.060	0.049	0.041	0.045	0.045	0.050	0.044	0.045
70	0.103	0.157	0.119	0.140	0.119	0.112	0.096	0.080	0.074	0.061	0.050	0.054	0.053	0.059	0.053	0.054
80	0.122	0.181	0.141	0.163	0.141	0.134	0.116	0.098	0.091	0.075	0.060	0.065	0.063	0.070	0.065	0.065
90	0.153	0.218	0.176	0.201	0.175	0.168	0.147	0.126	0.117	0.097	0.078	0.082	0.079	0.087	0.083	0.083

Table A.24: Mean Differences between Foreign-owned and Domestic-owned Firms by Size Bin

Notes: This table presents the mean difference in log value added, log wage bill per worker, log earnings, firm effects, and worker effects. Firms are grouped into log size bins, and means are computed within bins.

Outcome: (unit transformation)	**Earnings** (log)	**Income before Taxes** (log)	**Income after Taxes** (log)	**Tax Payments** (log)	**EITC Claims** (DHS transform)	**EITC Amount** (DHS transform)
2SLS Indirect Effect	0.466***	0.588***	0.538***	0.754***	-0.241	-0.312
	(0.138)	(0.149)	(0.144)	(0.232)	(0.182)	(0.203)
First Stage Coefficient	0.598***	0.597***	0.597***	0.600***	0.621***	0.621***
	(0.035)	(0.035)	(0.035)	(0.035)	(0.030)	(0.030)
First Stage F-statistics	297	291	291	295	421	421
Firm Observations (Millions)	45.9	34.8	34.7	35.4	16.3	16.3

Table A.25: Bartik IV Estimates for Various Income and Tax Measures

Notes: Controls are as in the main specifications for worker-weighted regressions described in the text. Standard errors are clustered at the CZ-year level. Firm-level observations are weighted by lagged firm size. The Davis et al. (1996, DHS) transform of x_t is given by $\frac{x_t - x_{t-1}}{(x_t + x_{t-1})/2}$ and is meant to approximate the log-difference when x has a large share of non-positive numbers and is thus not defined in the log transform. EITC claims and amounts are per-capita within the firm, and firms are equally weighted when using these per-capita outcomes.

Ownership	Mean Difference in:		
	Log Earnings	Firm Effects	Worker Effects
AE	0.155	0.043	0.117
AS	0.283	0.100	0.198
AU	0.268	0.107	0.159
BE	0.285	0.104	0.187
BR	0.171	0.054	0.112
CA	0.151	0.051	0.104
CH	-0.066	-0.041	-0.018
CO	0.052	0.030	0.025
DA	0.302	0.109	0.200
EI	0.355	0.124	0.239
FI	0.306	0.116	0.192
FR	0.214	0.082	0.141
GM	0.225	0.090	0.139
HK	0.121	0.041	0.082
IN	0.138	0.051	0.106
IS	0.256	0.097	0.178
IT	0.230	0.089	0.144
JA	0.175	0.079	0.098
KS	0.103	0.042	0.066
LU	0.298	0.104	0.192
MX	0.018	0.004	0.015
NL	0.279	0.101	0.182
NO	0.395	0.151	0.255
NZ	0.341	0.127	0.224
RS	0.106	0.025	0.096
SF	0.156	0.056	0.103
SN	0.174	0.054	0.131
SP	0.215	0.080	0.144
SW	0.302	0.115	0.191
SZ	0.276	0.105	0.174
TU	0.146	0.059	0.090
TW	0.001	0.005	-0.003
UK	0.275	0.099	0.185
VE	0.040	0.012	0.027

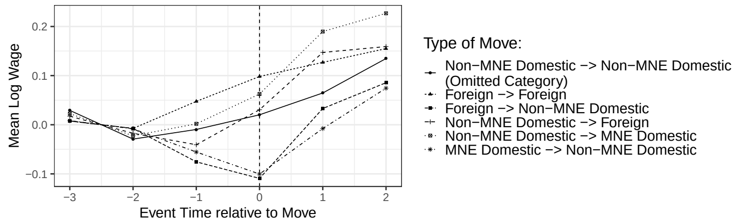
Table A.26: Differences between Foreign-owned and Domestic-owned Firms by Origin Country

Notes: This table presents the mean differences between log earnings, firm effects, and worker effects between the largest foreign ownership countries and the non-foreign-owned firms.

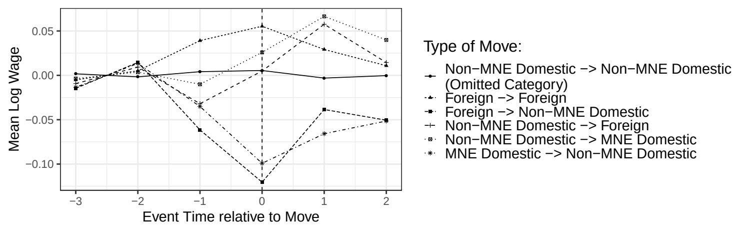
	Baseline	Check 1	Check 2	Check 3
Type of Move:				
Domestic to Foreign	0.063***	0.078***	0.059***	-0.002
	(0.002)	(0.002)	(0.003)	(0.004)
	(N=291,002)	(N=242,207)	(N=126,178)	(N=48,795)
Foreign to Domestic	-0.041***	-0.056***	-0.052***	0.014***
	(0.002)	(0.002)	(0.004)	(0.004)
	(N=210,862)	(N=172,896)	(N=46,729)	(N=37,966)
Foreign to Foreign	0.017***	0.020***	0.042***	0.006*
	(0.003)	(0.003)	(0.004)	(0.003)
	(N=246,192)	(N=246,192)	(N=128,396)	(N=246,192)
Domestic to Domestic	0	0	0	0
(Omitted Category)	(N=8,804,019)	(N=7,900,458)	(N=3,290,933)	(N=223,424)
Specification Details:				
Domestic Firms Restriction	All	Exclude MNE	Exclude MNE	Only include MNE
Type of Separation	All	All	Mass Layoff	All

Table A.27: Earnings growth for movers and stayers

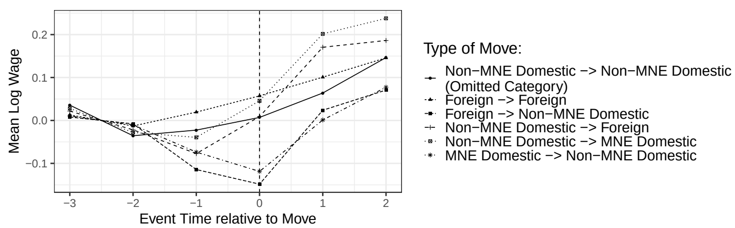
Notes: N denotes sample size. Standard errors in parentheses. The sample consists of only workers who were employed for two straight years at one firm followed by two straight years at a different firm. In the Baseline specification, the sample includes foreign and all domestic firms. In Check 1, we restrict the sample to domestic non-multinationals and foreign firms. In Check 2, we restrict the sample to domestic non-multinationals and foreign firms, and also restrict to workers who separated from a firm as part of a mass layoff. In Check 3, we restrict the sample to domestic multinationals and foreign firms.



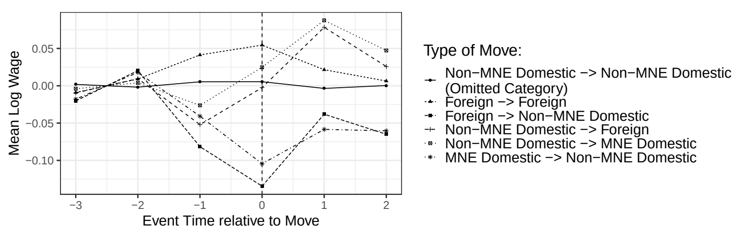
(a) Full sample, raw outcome



(b) Full sample, residual outcome



(c) Layoff sample, raw outcome



(d) Layoff sample, residual outcome

Figure A.28: Movers Event Study on Mean Earnings

Notes: In this figure, we compute the mean of the log earnings for various types of moves at event time zero, either when moving from a domestic-owned to a foreign-owned firm, or when moving from a foreign-owned to a domestic-owned firm. We compare these to workers who remain at the same domestic firm, as this is the omitted category in the regression for movers. We measure the outcome both as raw mean of log earnings, and residual mean of log earnings. We consider both the full sample of movers, and those who moved as part of a large layoff in which at least thirty percent of workers moved.

F Results from an Equilibrium Model of the Labor Market

F.1 Explanation of the Empirical Approach

In this appendix, we briefly describe the equilibrium model of the labor market. The full mathematical representation, formal assumptions, and equilibrium derivations are available from the authors and omitted here for brevity. The economy is composed of a large number of workers indexed by i and a large set of firms indexed by j. Each firm belongs to a market $r(j)$. Workers are heterogeneous both in preferences and productivity. Workers gain utility from after-tax wage earnings, the amenities available from firms and markets, and an idiosyncratic taste for firms. Firms differ not only in their amenities but also in terms of total factor productivity ("TFP") and technology. Workers view firms as imperfect substitutes, both due to differences in amenities offered by firms to all workers of a given quality, and due to differences in idiosyncratic tastes of workers for firms. We allow flexibility in the relationship between the amenities available from firms and markets, the market and firm-specific TFP, and the technology. Workers choose to work at the firm that maximizes utility. The idiosyncratic tastes of workers are not observed by the firm, while worker productivity is observed, and firms set profit-maximizing wages that depend on worker productivity. Under technical assumptions on the distribution of the workers' idiosyncratic preferences, we prove existence and uniqueness of the equilibrium in this economy.

Given the labor market equilibrium implied by the model, the underlying components of the model, such as TFP and amenities, are identified and can be estimated from moments of the data, the results from the BLM estimation, and the parameters of the tax function. Identification proofs and estimators are available from the authors and omitted for brevity. After estimating the underlying parameters of the model, we are able to predict from the estimated model the joint distribution of size, value added, firm effects, efficiency units of labor, and the wage bill. In Appendix Figure A.29, we provide visual evidence that the model predictions perform very well in replicating these moments, while Appendix Figure A.30 demonstrates that two estimators for the amenities offered by firms provide very similar results.[6]

First, we use the model to estimate the magnitude and sharing of rents between firms and workers, both at the firm and market level. Appendix Table A.28 presents the overall results, both when ignoring markets and when accounting for markets, while Appendix Table A.29 presents the heterogeneity in rents and rent-sharing across broad markets. Overall, we find that firm-level rents to workers are \$5,447 and firm-level rents to firms are \$5,780 in per-worker dollars, indicating that nearly 50% of rents accrue to workers. At the market-level, we find that rents to workers are \$7,331 while rents to firms are \$7,910, indicating that workers also share nearly 50% of rents at the market-level. Together, these estimates indicate that rents are large in the U.S. economy, suggesting that firms have substantial price-setting power in labor markets, but firms are not able to capture all of the rents.

[6] In the sample on which the model is estimated, there are 10,669,602 values of the time-varying firm premium, 61,670,459 values of worker quality, 1,953,915 values of amenity efficiency units and firm-specific TFP, 114,773 values of market-specific TFP, and 37,236,342 values of the preferences for amenities. The variances of these model components are 0.14 for amenity efficiency units, 0.04 for firm-specific TFP, 0.12 for market-specific TFP, and 0.20 for preferences for amenities.

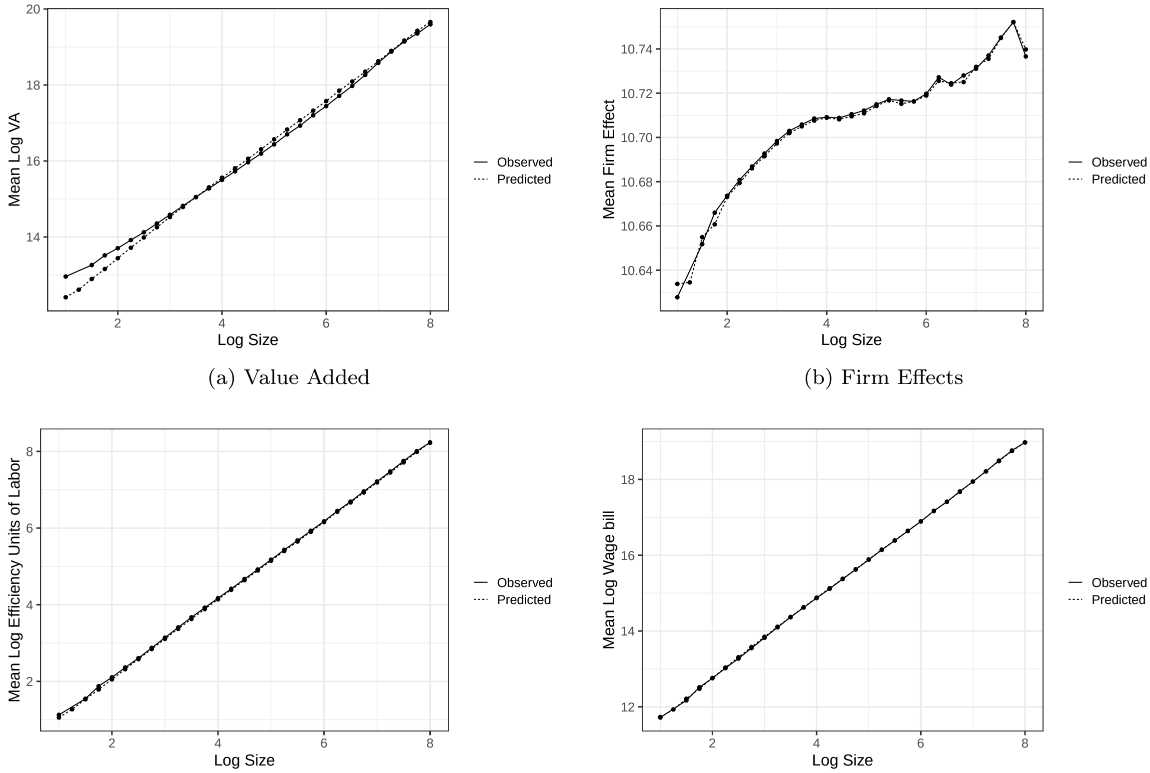
Second, we use the model to understand why firm premiums explain a small share of variance, even though the share of variance between firms is large. The results from these decompositions are reported in Table A.30. They suggest a lot of variation in amenities and productivity across firms. Interpreted in isolation, this heterogeneity predicts a large inequality contribution from firm effects. However, productive firms tend to have good amenities, which act as compensating differentials and push wages down in productive firms. As a result, firm effects explain only a few percent of the overall variation in log earnings. For example, firm effects within detailed markets explain 3.1 percent of the variation in log earnings, which is much less than predicted by the variances of firm productivity (8.6 percent) and amenities (7.1 percent).

In Appendix Figure A.31, we estimate compensating differentials directly. For two randomly drawn firms, the one with worse amenities can be expected to pay an additional 18 percent in order to convince marginal workers (of average quality) to accept the job. There is, however, considerable heterogeneity in the compensating differentials according to worker quality. The upward sloping solid line shows how the expected compensating differential varies with worker quality. For high quality workers (95 percentile in the national distribution), the expected compensating differentials are as large as 30 percent. By comparison, marginal workers of low quality (5 percentile in the national distribution) require less than 10 percent additional pay to work in the firm with the unfavorable amenities.

Third, we use the equilibrium model to investigate how sorting would change if we were to "shrink" the differences across firms in amenities (denoted by g_j) or in productivity interaction parameters (denoted by θ_j). Appendix Figure A.32 demonstrates how the sorting correlation and the share of log earnings variance explained by sorting varies as the amenity and productivity differences are shrunk, and Appendix Figure A.33 provides a detailed examination of how sorting responds to shrinking. We see that shrinking the g_j leads to a greater role for sorting in earnings inequality while shrinking θ_j results in a lesser role for sorting.

Fourth, in Appendix Table A.31, we compare the monopsonistic labor market to a counterfactual economy which differs in two ways. First, we eliminate the tax wedge in the first order condition by setting the tax progressivity $(1 - \lambda)$ equal to zero. Second, we remove the labor wedges in the first order conditions of the firms. Results are displayed for output, welfare, the sorting correlation, the mean labor wedge, and worker rents. They suggest the monopsonistic labor market create significant misallocation of workers to firms. Eliminating labor and tax wedges increase total welfare by 5 percent and total output by 3 percent. We also find that removing these wedges would increase the sorting of better workers to higher paying firms and lower the rents that workers earn from ongoing employment relationships.

F.2 Tables and Figures



(a) Value Added

(b) Firm Effects

(c) Efficiency Units of Labor

(d) Wage Bill

Figure A.29: Fit of the Model for Untargeted Moments

Notes: In this figure, we compare the observed and the predicted values of firm effects, value added, efficiency units of labor, and wage bill. We make this comparison separately according to the actual and predicted firm size.

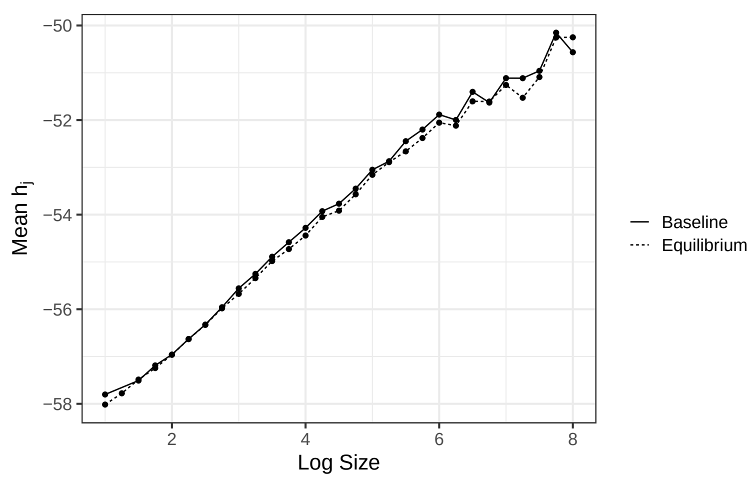


Figure A.30: Fit of h_j with and without Imposing the Equilibrium Constraint

Notes: In this figure, we plot the mean of h_j across log size bins. We compare the baseline estimates of h_j from the equation for firm wage premiums, versus those estimated using the equilibrium constraint by solving the fixed-point definition of h_j as a function of amenity preferences.

	Rents and Rent-shares		
	Firm Only	**Accounting for Markets**	
	Firm-level	**Firm-level**	**Market-level**
Workers' Rents:			
Per-worker Dollars	5,875	5,447	7,331
	(284)	(395)	(1,234)
Share of Earnings	14%	13%	18%
	(1%)	(1%)	(3%)
Firms' Rents:			
Per-worker Dollars	5,932	5,780	7,910
	(709)	(1,547)	(1,737)
Share of Profits	11%	11%	15%
	(1%)	(3%)	(3%)
Workers' Share of Rents	50%	49%	48%
	(2%)	(4%)	(3%)

Table A.28: Estimates of rents and rent sharing (national averages)

Notes: This table displays our main results on rents and rent-sharing. Column 1 presents results from the specification which imposes $\Upsilon = \gamma$, $\rho_r = 1$, and $\alpha_r = \alpha$ ("Firm only"), while columns 2-3 report results from the specification which allows Υ to differ from γ, and for ρ_r and α_r to vary across broad markets ("Accounting for Markets"). Standard errors are estimated using 40 block bootstrap draws in which the block is taken to be the market.

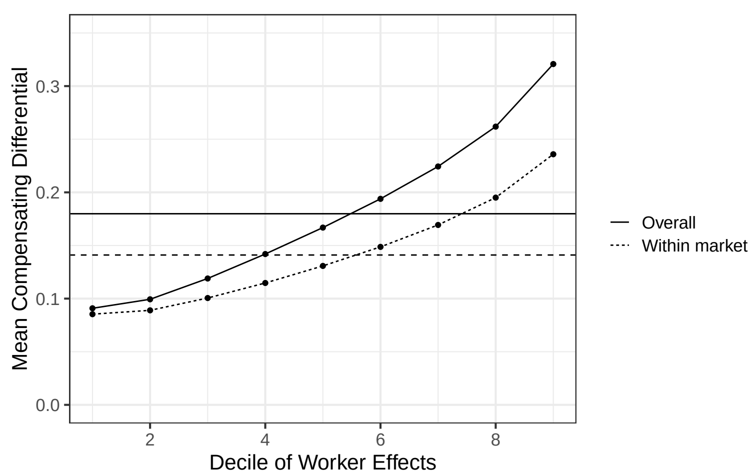


Figure A.31: Compensating differentials

Notes: In this figure, we plot mean compensating differentials overall and within market. To do so, we randomly draw a pair of firms (j, j') with probability proportional to firm size. Each j' is drawn from the full set of firms when estimating overall compensating differentials and from the set of firms in the same market as j when estimating within market compensating differentials. Then, we estimate the compensating differential between j and j' for a worker of given quality $x_i = x$ by $\psi_j + x\theta_j - \psi_{j'} - x\theta_{j'}$. This figure plots the mean absolute value of the compensating differentials across deciles of the x_i distribution, where the horizontal lines denote means across the distribution of x_i.

	Goods				Services			
	Midwest	Northeast	South	West	Midwest	Northeast	South	West
Panel A.	**Model Parameters**							
Idyosinctratic taste parameter (β^{-1})	0.200							
	(0.044)							
Taste correlation parameter (ρ)	0.844	0.694	0.719	0.924	0.649	0.563	0.744	0.619
	(0.179)	(0.153)	(0.160)	(0.182)	(0.141)	(0.109)	(0.246)	(0.117)
Returns to scale ($1-\alpha$)	0.746	0.764	0.863	0.949	0.753	0.740	0.814	0.752
	(0.016)	(0.013)	(0.017)	(0.019)	(0.013)	(0.015)	(0.036)	(0.015)
Panel B.	**Firm-level Rents and Rent Shares**							
Workers' Rents:								
Per-worker Dollars	6,802	6,681	5,737	8,906	4,234	4,847	5,009	4,805
	(770)	(723)	(720)	(867)	(502)	(803)	(1,295)	(684)
Share of Earnings	16%	13%	14%	17%	12%	11%	14%	12%
	(2%)	(1%)	(2%)	(2%)	(1%)	(2%)	(4%)	(2%)
Firms' Rents:								
Per-worker Dollars	4,041	4,198	7,465	20,069	3,531	3,097	6,915	3,018
	(1,243)	(1,130)	(2,681)	(6,323)	(1,004)	(1,305)	(5,650)	(1,060)
Share of Profits	8%	7%	17%	52%	6%	5%	12%	6%
	(3%)	(2%)	(6%)	(16%)	(2%)	(2%)	(10%)	(2%)
Workers' Share of Rents	63%	61%	43%	31%	55%	61%	42%	61%
	(4%)	(4%)	(5%)	(4%)	(4%)	(5%)	(9%)	(5%)
Panel C.	**Market-level Rents and Rent Shares**							
Workers' Rents:								
Per-worker Dollars	7,837	9,102	7,572	9,506	6,115	7,935	6,422	7,230
	(1,319)	(1,532)	(1,274)	(1,600)	(1,029)	(1,335)	(1,081)	(1,217)
Share of Earnings	18%	18%	18%	18%	18%	18%	18%	18%
	(3%)	(3%)	(3%)	(3%)	(3%)	(3%)	(3%)	(3%)
Firms' Rents:								
Per-worker Dollars	4,940	6,311	10,000	20,846	5,734	5,897	9,363	5,153
	(1,140)	(1,350)	(2,267)	(5,787)	(1,351)	(1,786)	(4,218)	(1,433)
Share of Profits	10%	11%	23%	54%	10%	9%	16%	10%
	(2%)	(2%)	(5%)	(15%)	(2%)	(3%)	(7%)	(3%)
Workers' Share of Rents	61%	59%	43%	31%	52%	57%	41%	58%
	(3%)	(3%)	(4%)	(5%)	(3%)	(4%)	(8%)	(4%)

Table A.29: Broad Market Heterogeneity in Model Parameters and Rent Sharing Estimates

Notes: This table displays our heterogeneity in the estimated model parameters and rents and rent-sharing. These results correspond to the specification which allows Υ to differ from γ, and for ρ_r and α_r to vary across broad markets. Standard errors are estimated using 40 block bootstrap draws in which the block is taken to be the market.

	Between Broad Markets	**Within Broad Markets**	
		Between Detailed Markets	**Within Detailed Markets**
Total	0.4%	2.0%	3.1%
Decomposition:			
Amenity Differences	15.9%	7.8%	7.1%
TFP Differences	15.5%	11.9%	8.6%
Amenity-TFP Covariance	-31.1%	-17.7%	-12.6%

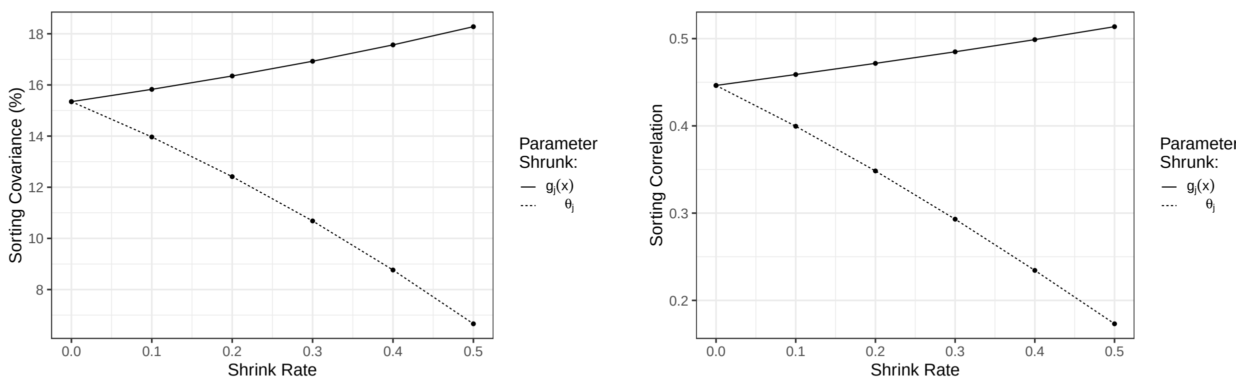
Table A.30: Decomposition of the Variation in Firm Premiums

Notes: This table displays our estimates of the decomposition of time-varying firm premium variation in three levels: variation between broad markets, between detailed markets (within broad markets), and between firms (within detailed markets). Broad markets are defined as the combination of sector times region, and detailed markets are defined as the combination of industry times commuting zone. We decompose the variation in time-varying firm premiums into the contributions from amenity differences, TFP differences, and the covariance between amenity and TFP differences. All variances are expressed as shares of log earnings variance.

		(1) Monopsonistic Labor Market	(2) No Labor or Tax Wedges	Difference between (1) and (2)
Log of Expected Output	$\log \mathbb{E}[Y_{jt}]$	11.38	11.41	0.03
Total Welfare (log dollars)		12.16	12.21	0.05
Sorting Correlation	$Cor(\psi_{jt}, x_i)$	0.44	0.47	0.03
Labor Wedges	$1 + \frac{\rho_r}{\beta\lambda}$	1.15	1.00	-0.15
Worker Rents (as share of earnings):				
Firm-level	$\frac{\rho_r}{\rho_r + \beta\lambda}$	13.3%	12.3%	-1.0%
Market-level	$\frac{1}{1+\beta\lambda}$	18.0%	16.7%	-1.3%

Table A.31: Consequences for Worker Allocation and Outcomes of Eliminating Tax and Labor Wedges

Notes: This table compares the monopsonistic labor market to a counterfactual economy which differs in two ways. First, we eliminate the tax wedge in the first order condition by setting the tax progressivity $(1-\lambda)$ equal to zero. Second, we remove the labor wedges in the first order conditions of the firms by setting τ_r equal to the labor wedge $1 + \frac{\rho_r}{\lambda\beta}$ in each market r. After changing these parameters of the model, we solve for the new equilibrium allocation and outcomes, including wages, output and welfare. Results are displayed for output, welfare, the sorting correlation, the mean labor wedge, and worker rents.

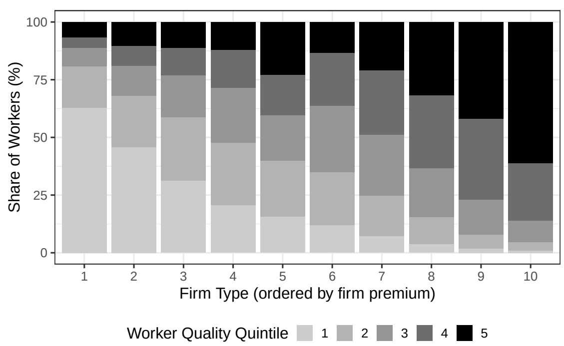


(a) Sorting

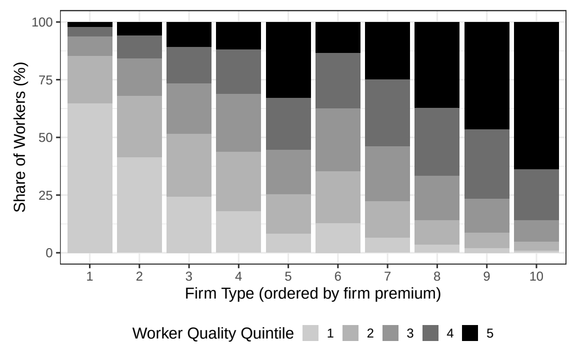
(b) Sorting Correlation

Figure A.32: Worker sorting with counterfactual values of $g_j(x)$ and θ_j

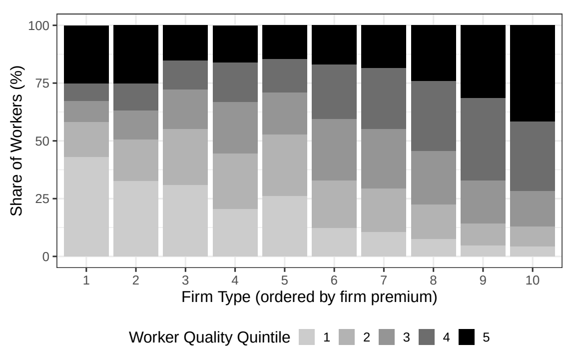
Notes: In this figure, we reduce the heterogeneity across firms in amenities or production complementarities by replacing either $g_j(x)$ with $(1-s)\, g_j(x) + s\bar{g}_j$ or θ_j with $(1-s)\,\theta_j + s\bar{\theta}$, where $\bar{g}_j = \mathbb{E}_x\,[g_j(x)]$, $\bar{\theta} = \mathbb{E}\,[\theta_j]$. Here, $s \in [0,1]$ is the shrink rate with $s = 0$ corresponding to the baseline model. We report the share of log earnings variance explained by sorting (subfigure a) and the sorting correlation (subfigure b).



(a) Baseline Equilibrium



(b) Shrink $g_j(x)$



(c) Shrink θ_j

Figure A.33: Actual and counterfactual composition of the workforce by firm types

Notes: In this figure, we reduce the heterogeneity across firms in amenities or production complementarities by replacing either $g_j(x)$ with $(1-s)\, g_j(x) + s\bar{g}_j$ or θ_j with $(1-s)\, \theta_j + s\bar{\theta}$, where $\bar{g}_j = \mathbb{E}_x\left[g_j(x)\right]$, $\bar{\theta} = \mathbb{E}\left[\theta_j\right]$. Here, $s \in [0, 1]$ is the shrink rate with $s = 0$ corresponding to the baseline model. We report the quality of the workforce by firm type in the baseline economy with $s = 0$ (subfigure a) and the counterfactual economies with $s = \frac{1}{2}$ for either amenities (subfigure b) or production complementarities (subfigure c).